

MIND
A QUARTERLY REVIEW
OF
PSYCHOLOGY AND PHILOSOPHY

I.—WITTGENSTEIN'S LECTURES
IN 1930-33

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III

(B) In the case of Logic, there were two most important matters with regard to which he said that the views he had held when he wrote the *Tractatus* were definitely wrong.

(1) The first of these concerned what Russell called "atomic" propositions and he himself in the *Tractatus* had called "Elementarsätze". He said in (II) that it was with regard to "elementary" propositions and their connexion with truth-functions or "molecular" propositions that he had had to change his opinions most; and that this subject was connected with the use of the words "thing" and "name". In (III) he began by pointing out that neither Russell nor he himself had produced any examples of "atomic" propositions; and said that there was something wrong indicated by this fact, though it was difficult to say exactly what. He said that both he and Russell had the idea that non-atomic propositions could be "analysed" into atomic ones, but that we did not yet know what the analysis was: that, e.g. such a proposition as "It is raining" might, if we knew its analysis, turn out to be molecular, consisting, e.g. of a conjunction of "atomic" propositions. He said that in the *Tractatus* he had objected to Russell's assumption that there certainly were atomic propositions which asserted two-termed relations—that he had refused to prophesy as to what would be the result of an analysis, if one were made, and that it might turn out that no atomic proposition asserted less than e.g. a

four-termed relation, so that we could not even talk of a two-termed relation. His present view was that it was senseless to talk of a "final" analysis, and he said that he would now treat as atomic all propositions in the expression of which neither "and", "or", nor "not" occurred, nor any expression of generality, provided we had not expressly given an exact definition, such as we might give of "It's rotten weather", if we said we were going to use the expression "rotten" to mean "both cold and damp".

In saying this he seemed to me to be overlooking both the fact that a man often says that he is going to use an expression in a certain definite way and then does not in fact so use it, and also the fact that many common words, e.g. father, mother, sister, brother, etc., are often so used that such a sentence as "This is my father" undoubtedly expresses a molecular proposition, although a person who so uses it has never expressly stated that he will so use it. These two facts, however, of course, do not prove that he was wrong in thinking that it is senseless to talk of a "final" or "ultimate" analysis.

(2) The second important logical mistake which he thought he had made at the time when he wrote the *Tractatus* was introduced by him in (III) in connexion with the subject of "following" (by which he meant, as usual, *deductive* following or "entailment"—a word which I think he actually used in this discussion) from a "general" proposition to a particular instance and from a particular instance to a "general" proposition. Using the notation of *Principia Mathematica*, he asked us to consider the two propositions " $(x).fx$ entails fa " and " fa entails $(\exists x).fx$ ". He said that there was a temptation, to which he had yielded in the *Tractatus*, to say that $(x).fx$ is identical with the logical product " $fa.fb.fc\dots$ ", and $(\exists x).fx$ identical with the logical sum " $fa \vee fb \vee fc \dots$ ", but that this was in both cases a mistake. In order to make clear exactly where the mistake lay, he first said that in the case of such a universal proposition as "Everybody in this room has a hat" (which I will call "A"), he had known and actually said in the *Tractatus*, that, even if Smith, Jones and Robinson are the only people in the room, the logical product "Smith has a hat, Jones has a hat and Robinson has a hat" cannot possibly be identical with A, because in order to get a proposition which entails A, you obviously have to add "and Smith, Jones and Robinson are the only people in the room". But he went on to say that if we are talking of "individuals" in Russell's sense (and he actually

here mentioned atoms as well as colours, as if they were "individuals" in this sense), the case is different, because, in that case, there is no proposition analogous to "Smith, Jones and Robinson are the only people in the room". The class of things in question, if we are talking of "individuals", is, he said, in this case, determined not by a proposition but by our "dictionary": it is "defined by grammar". *E.g.* he said that the class "primary colour" is "defined by grammar", not by a proposition; that there is no such proposition as "red is a primary colour", and that such a proposition as "In this square there is one of the primary colours" really is identical with the logical sum "In this square there is either red or green or blue or yellow"; whereas in the case of Smith, Jones and Robinson, there is such a proposition as "Smith is in this room" and hence also such a proposition as "Smith, Jones and Robinson are the only people in this room". He went on to say that one great mistake which he made in the *Tractatus* was that of supposing that in the case of *all* classes "defined by grammar", general propositions were identical either with logical products or with logical sums (meaning by this logical products or sums of the propositions which are values of f_x) as, according to him, they really are in the case of the class "primary colours". He said that, when he wrote the *Tractatus*, he had supposed that *all* such general propositions were "truth-functions"; but he said now that in supposing this he was committing a fallacy, which is common in the case of Mathematics, *e.g.* the fallacy of supposing that $1 + 1 + 1 \dots$ is a sum, whereas it is only a *limit*, and that $\frac{dx}{dy}$ is a quotient, whereas it also is only a *limit*. He said he had been misled by the fact that $(x).f_x$ can be replaced by $fa.fb.fc\dots$, having failed to see that the latter expression is not always a logical product: that it is only a logical product if the dots are what he called "the dots of laziness", as where we represent the alphabet by "A, B, C . . .", and therefore the whole expression can be replaced by an enumeration; but that it is not a logical product where, *e.g.* we represent the cardinal numbers by 1, 2, 3 . . ., where the dots are not the "dots of laziness" and the whole expression can not be replaced by an enumeration. He said that, when he wrote the *Tractatus*, he would have defended the mistaken view which he then took by asking the question: How can $(x).f_x$ possibly entail fa , if $(x).f_x$ is not a logical product? And he said that the answer to this question is that where $(x).f_x$ is not a logical product, the proposition " $(x).f_x$ entails fa " is "taken as a primary

proposition ", whereas where it is a logical product this proposition is deduced from other primary propositions.

The point which he here made in saying that where we talk of the cardinal numbers we are not talking of a logical product was a point which he had made earlier, in (I), though he did not there point out that in the *Tractatus* he had made the mistake of supposing that an infinite series was a logical product—that it could be enumerated, though we were unable to enumerate it. In this passage in (I) he began by saying that by the proposition "there are an infinite number of shades of grey between black and white" we "mean something entirely different" from what we mean by e.g. "I see three colours in this room", because, whereas the latter proposition can be verified by counting, the former can not. He said that "There are an infinite number" does not give an answer to the question "How many are there?" whereas "There are three" does give an answer to this question. He went on to discuss infinite divisibility in the case of space, and said (as I have already mentioned p. 296), that the "linguistic expression" of "This line can be bisected" was "The words 'This line has been bisected' have sense", but that the "linguistic expression" of "This line can be infinitely divided" is certainly not "The words 'This line has been infinitely divided' have sense". He said that if we express "has been bisected", "has been trisected", "has been quadrisectioned", etc., by $f(1+1)$, $f(1+1+1)$, $f(1+1+1+1)$, etc., we see that an internal relation holds between successive members of this series and that the series has no end; and he concluded by saying that the "linguistic expression" of an infinite possibility is an infinite possibility in language. He also pointed out that $\Sigma 1 + \frac{1}{2} + \frac{1}{4} \dots$ approaches a limit, whereas a logical product does not approach any limit. And he said finally that the cases to which the *Principia* notations $(x) \cdot \phi x$ and $(\exists x) \cdot \phi x$ apply, i.e. cases in which the former can be regarded as a logical product and the latter as a logical sum of propositions of the form ϕa , ϕb , ϕc , etc., are comparatively rare; that oftener we have propositions, such as "I met a man", which do not "presuppose any totality"; that the cases to which the *Principia* notations apply are only those in which we could give proper names to the entities in question; and that giving proper names is only possible in very special cases.

Besides these two cardinal cases, in which he said that the views which he had held at the time when he wrote the *Tractatus* were certainly wrong, I think that the chief logical points which he made were as follows.

(3) One point which he made was that Russell was quite wrong in supposing that, if expressions of the form " $p \supset q$ " are used with the meaning which is given to " \supset " in *Principia Mathematica*, then it follows that from a false proposition we can infer every other proposition, and that from a true one we can infer every other true one. He said that Russell's holding this false opinion was partly due to his supposing that " $p \supset q$ " can be translated by "If p , then q ". He said that we never use "If p , then q " to mean merely what is meant by " $p \supset q$ "; and that Russell had admitted this, but still maintained that in the case of what he called "formal implications", i.e. propositions of the form $(x). \phi x \supset \psi x$, such a proposition can be properly translated by "If . . ., then . . .". Wittgenstein said that this also was a mistake, giving as a reason that if, e.g. we substitute "is a man" for ϕ and "is mortal" for ψ , then the mere fact that there were no men would verify $(x). \phi x \supset \psi x$, but that we never so use "If . . ., then . . ." that the mere fact that there were no men would verify "If anything is a man, then that thing is mortal".

(4) He also, on more than one occasion, said something about Sheffer's "stroke notation", and, on one occasion, about Tarski's "3-valued" Logic.

About the former he said that it resembled what are called mathematical "discoveries" in respect of the fact that Sheffer had no rule for discovering an answer to the question "Is there only one logical constant?" whereas there is a rule for discovering, e.g. the answer to a multiplication sum. He said that, where there is no rule, it is misleading to use the word "discovery", though this is constantly done. He said that Russell or Frege might quite well have used the expression " p/q " as short for " $\sim p \cdot \sim q$ ", and yet still maintained that they had two primitive ideas, "and" and "not", and not one only. Plainly, therefore, he thought that Sheffer, though he admitted that Sheffer had actually defined " p/q " as meaning " $\sim p \cdot \sim q$ ", had done something else. But what else? He said that Sheffer's "discovery" consisted in finding a "new aspect" of certain expressions. But I am sorry to say that I did not and do not understand what he meant by this.

On Tarski's 3-valued Logic he said that it was all right "as a calculus"—that Tarski had really "discovered" a new calculus. But he said that "true" and "false" could not have in it the meaning which they actually have; and he particularly emphasized that Tarski had made the mistake of supposing that his third value, which he called "doubtful", was identical with what we ordinarily mean by "doubtful".

(C) Of problems which are specifically problems in the philosophy of Mathematics, I think that those which he most discussed are the three following. But in this case I should like to remind the reader of what I said in my first article (p. 5) that I cannot possibly mention nearly everything which he said, and that it is possible that some things which I omit were really more important than what I mention; and also to give the warning that in this case it is particularly likely that I may have misunderstood or may misrepresent him, since my own knowledge of Mathematics is very small. But I think that what I say will at least give some idea of the *kind* of questions which he was eager to discuss.

(1) In (I) he said that there were two very different kinds of proposition used in Mathematics, "neither of them at all like what are usually called propositions". These were (1) propositions proved by a chain of equations, in which you proceed from axioms to other equations, by means of axioms, and (2) propositions proved by "mathematical induction". And he added in (III) that proofs of the second kind, which he there called "recursive proofs", are not proofs in the same *sense* as are proofs of the first kind. He added that people constantly commit the fallacy of supposing that "true", "problem" "looking for", "proof" always mean the same, whereas in fact these words "mean entirely different things" in different cases.

As an example of a proposition of the second kind he took the Associative Law for the addition of numbers, namely, " $a + (b + c) = (a + b) + c$ "; and he discussed the proof of this proposition at considerable length on two separate occasions, first in (I) and then later in (III). On both occasions he discussed a proof of it given by Skolem, though in (I) he did not expressly say that the proof discussed was Skolem's. He said in (I) that the proof seemed to assume at one point the very proposition which it professed to prove, and he pointed out in (III) that in one of the steps of his proof Skolem did actually assume the Associative Law. He said that since Skolem professed to be giving a proof, one would have expected him to prove it from other formulae, but that in fact the proof begins in an entirely different way, namely with a definition—the definition " $a + (b + 1) = (a + b) + 1$ "; and he maintained both in (I) and in (III) that it was quite unnecessary for Skolem to assume the Associative Law in one step of his proof, saying in (I) that the proof "really rests entirely on the definition", and in (III) that you don't in fact use the Associative Law in the proof at

all. He wrote the proof "in his own way" in order to show this, saying that if you write the definition in the form " $\phi 1 = \psi 1$ ", then all that is proved is the two formulae (a) $\phi(c + 1) = \phi c + 1$ and (b) $\psi(c + 1) = \psi c + 1$, and that to prove these two formulae is the same thing as what is called "proving the Associative Law for all numbers". He went on to say that the fact that this proof proves all we want "shows that we are not dealing with an extension at all"; that instead of talking of a *finite part* of the series "1, 2, 3 . . .", on the one hand, and of the *whole* series on the other hand, we should talk of a bit of the series and of the *Law which generates it*; that proving the Associative Law "for all numbers" can't mean the same sort of thing as proving it, e.g. for three numbers, since, in order to do this latter, you would have to give a separate proof for each of the three; and that what we have in the proof is a general *form* of proof for *any* number. Finally he said that the generality which is misleadingly expressed by saying that we have proved the Associative Law for "all cardinals", really comes in in the definition, which might have been written in the form of a series, *viz.* " $1 + (1 + 1) = (1 + 1) + 1$ " " $1 + (2 + 1) = (1 + 2) + 1$ " " $2 + (1 + 1) = (2 + 1) + 1$ " *and so on*; and that this series is not a logical product of which the examples given are a part, but a *rule*, and that "the examples are only there to explain the rule".

(2) Another problem in the philosophy of Mathematics, which he discussed on no less than three separate occasions, was what we are to say of the apparent question "Are there anywhere in the development of π three consecutive 7's?" (Sometimes he took the question "Are there five consecutive 7's?" instead of "Are there three?") He first dealt with this apparent question in (I), in connexion with Brouwer's view that the Law of Excluded Middle does not apply to some mathematical propositions; i.e. that some mathematical propositions are neither true nor false; that there is an alternative to being either true or false, *viz.* being "undecidable". And on this occasion he said that the words "There are three consecutive 7's in the development of π " are nonsense, and that hence not only the Law of Excluded Middle does not apply in this case, but that no laws of Logic apply in it; though he admitted that if someone developed π for ten years and actually found three consecutive 7's in the development, this would prove that there were three consecutive 7's *in a ten years' development*, and seemed to be admitting, therefore, that it is possible that there might be. The next time he discussed the question, early in (III), he said that if anyone actually found three consecutive 7's this would prove that there are, but that

if no one found them that wouldn't prove that there are not ; that, therefore, it is something for the truth of which we have provided a test, but for the falsehood of which we have provided none ; and that therefore it must be a quite different sort of thing from cases in which a test for both truth and falsehood is provided. He went on to discuss the apparent question in a slightly new way. He said we seem to be able to define π' as the number which, if there are three consecutive 7's in the development of π , differs from π in that, in the place in which three consecutive 7's occur in π , there occur in it three consecutive 1's instead, but which, if there are not, does not differ from π at all ; and that we seem to be able to say that π' , so defined, either is identical with π or is not. But he said here that, since we have no way of finding out whether π' is identical with π or not, the question whether it is or not "has no meaning" ; and, so far as I can see, this entails the same view which he had expressed in (I), *viz.* that the words "There are *not* three consecutive 7's anywhere in the development of π " have no meaning, since, if these words had a meaning, it would seem to follow that " $\pi' = \pi$ " also has one, and that therefore the question "Is π' identical with π ?" also has one. In the second passage in (III) in which he discussed this apparent question he expressly said that though the words (1) "There are five consecutive 7's in the first thousand digits of π " have sense, yet the words (2) "There are five consecutive 7's *somewhere* in the development" have none, adding that "we can't say that (2) makes sense because (2) follows from (1)". But in the very next lecture he seemed to have changed his view on this point, since he there said "We ought not to say 'There are five 7's in the development' has no sense", having previously said "It has whatever sense its grammar allows", and having emphasized that "it has a very curious grammar" since "it is compatible with there not being five consecutive 7's in any development you can give". If it has sense, although a "very curious" one, it does presumably express a proposition to which the Law of Excluded Middle and the other rules of Formal Logic do apply ; but Wittgenstein said nothing upon this point. What he did say was that "All big mathematical problems are of the nature of 'Are there five consecutive 7's in the development of π ?'" and that "they are therefore quite different from multiplication sums, and not comparable in respect of difficulty".

He said many other things about this question, but I cannot give them all, and some of them I certainly did not and do not understand. But one puzzling thing which he seemed to say in (III) was that, if we express the proposition that there is, in

the development of π , a number of digits which is immediately followed by five consecutive 7's, by " $(\exists n) . fn$ ", then there are two conceivable ways of proving $(\exists n) . fn$, namely, (1) by *finding* such a number, and (2) by proving that $\sim(\exists n) . fn$ is self-contradictory; but that the $(\exists n) . fn$ proved in the latter way could not be the same as that proved in the former. In this connexion he said that there is no "opposite" to the first method of proof. He said also that " $\exists n$ " means something different where it is possible to "look for" a number which proves it, from what it means where this is not possible; and, generally, that "The proof of an existence theorem gives the meaning of 'existence' in that theorem", whereas the meaning of "There's a man in the next room" does not depend on the method of proof.

(3) This last problem is connected, and was connected by him, with a general point which he discussed more than once in connexion with the question "How can we look for a method of trisecting an angle by rule and compasses, if there is no such thing?" He said that a man who had spent his life in trying to trisect an angle by rule and compasses would be inclined to say "If you understand both what is meant by 'trisection' and what is meant by 'bisection by rule and compasses', you must understand what is meant by 'trisection by rule and compasses'" but that this was a mistake; that we can't imagine trisecting an angle by rule and compasses, whereas we can imagine dividing an angle into eight equal parts by rule and compasses; that "looking for" a trisection by rule and compasses is not like "looking for" a unicorn, since "There are unicorns" has sense, although in fact there are no unicorns, whereas "There are animals which show on their foreheads a construction by rule and compasses of the trisection of an angle" is just nonsense like "There are animals with three horns, but also with only one horn": it does not give a description of any possible animal. And Wittgenstein's answer to the original question was that by proving that it is impossible to trisect an angle by rule and compasses "we change a man's idea of trisection of an angle" but that we should say that what has been proved impossible is the very thing which he had been trying to do, because "we are willingly led in this case to identify two different things". He compared this case to the case of calling what he was doing "philosophy", saying that it was not the same kind of thing as Plato or Berkeley had done, but that we may feel that what he was doing "takes the place" of what Plato and Berkeley did, though it is really a different thing. He illustrated the same

point in the case of the "construction" of a regular pentagon, by saying that if it were proved to a man who had been trying to find such a construction that there isn't any such thing, he would say "That's what I was trying to do" because "his idea has shifted on a rail on which he is ready to shift it". And he insisted here again that (a) to have an idea of a regular pentagon and (b) to know what is meant by constructing by rule and compasses, *e.g.* a square, do not in combination enable you to know what is meant by constructing, by rule and compasses, a regular pentagon. He said that to explain what is meant by "construction" we can give two series of "constructions", *viz.* (a) equilateral triangle, regular hexagon, etc., and (b) square, regular octagon, etc., but that neither of these would give meaning to the construction of a regular pentagon, since they don't give any rule which applies to the number 5. He said that in a sense the result wanted is clear, but the means of getting at it is not; but in another sense, the result wanted is itself not clear, since "constructed pentagon" is not the same as "measured pentagon" and that whether the same figure will be both "depends on our physics": why we call a construction a construction of a regular pentagon is "because of the physical properties of our compasses, etc."

In (I) he had said that in the case of Logic and Mathematics (and "Sense-data") you can't know the same thing in two independent ways; and that it was in the case of "hypotheses" and *nowhere else*, that there are different evidences for the same thing. But in (III) he said that even in the case of hypotheses, *e.g.* the proposition that there is a cylindrical object on the mantel-piece, he himself preferred to say that if the evidence was different, the proposition was also different, but that "you can say which you please". He did not say whether, in the case of Logic and Mathematics also, he now held that "you can say which you please".

(D) He spent, as I have said in my first article (p. 5), a great deal of time on this discussion, and I am very much puzzled as to the meaning of much that he said, and also as to the connexion between different things which he said. It seems to me that his discussion was rather incoherent, and my account of it must be incoherent also, because I cannot see the connexion between different points which he seemed anxious to make. He said very early in the discussion that the whole subject is "extraordinarily difficult" because "the whole field is full of misleading notations"; and that its difficulty was shown by the fact that the

question at issue is the question between Realists, Idealists and Solipsists. And he also said, more than once, that many of the difficulties are due to the fact that there is a great temptation to confuse what are merely experiential propositions, which might, therefore, not have been true, with propositions which are necessarily true or are, as he once said, "tautological or grammatical statements". He gave, as an instance of a proposition of the latter sort, "I can't feel your toothache", saying that "If you feel it, it isn't mine" is a "matter of grammar", and also that "I can't feel your toothache" means the same as "I feel your toothache' has no sense"; and he contrasted this with "I hear my voice coming from somewhere near my eyes", which he said we think to be necessary, but which in fact is not necessary "though it always happens". In this connexion he gave the warning "Don't be prejudiced by anything which is a fact, but which *might* be otherwise". And he seemed to be quite definite on a point which seems to me certainly true, *viz.* that I might see without physical eyes, and even without having a body at all; that the connexion between seeing and physical eyes is merely a fact learnt by experience, not a necessity at all; though he also said that "the visual field" has certain internal properties, such that you can describe the motion of certain things in it as motions towards or away from "your eye"; but that here "your eye" does not mean your physical eye, nor yet anything whatever which is *in* the visual field. He called "your eye", in this sense, "the eye of the visual field", and said that the distinction between motion towards it and away from it was "on the same level" as "the distinction between 'curved' and 'straight'".

However, he began the discussion by raising a question, which he said was connected with Behaviourism, namely, the question "When we say 'He has tooth-ache' is it correct to say that his tooth-ache is only his behaviour, whereas when I talk about my tooth-ache I am not talking about my behaviour?"; but very soon he introduced a question expressed in different words, which is perhaps not merely a different formulation of the same question, *viz.* "Is another person's toothache 'tooth-ache' in the same sense as mine?" In trying to find an answer to this question or these questions, he said first that it was clear and admitted that what verifies or is a criterion for "I have tooth-ache" is quite different from what verifies or is a criterion for "He has tooth-ache", and soon added that, since this is so, the *meanings* of "I have toothache" and "he has tooth-ache" must be different. In this connexion he said later, first, that the

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meaning of "verification" is different, when we speak of verifying "I have" from what it is when we speak of verifying "He has", and then, later still, that there is no such thing as a verification for "I have", since the question "How do you know that you have tooth-ache?" is nonsensical. He criticized two answers which might be given to this last question by people who think it is not nonsensical, by saying (1) that the answer "Because I feel it" won't do, because "I feel it" means the same as "I have it", and (2) that the answer "I know it by inspection" also won't do, because it implies that I can "look to see" whether I have it or not, whereas "looking to see whether I have it or not" has no meaning. The fact that it is nonsense to talk of verifying the fact that I have it, puts, he said, "I have it" on "a different level" in grammar from "he has it". And he also expressed his view that the two expressions are on a different grammatical level by saying that they are not both values of a single propositional function " x has tooth-ache"; and in favour of this view he gave two definite reasons for saying that they are not, namely, (1) that "I don't know whether I have tooth-ache" is always absurd or nonsense, whereas "I don't know whether he has tooth-ache" is not nonsense, and (2) that "It seems to me that I have tooth-ache" is nonsense, whereas "It seems to me that he has" is not.

He said, that when he said this, people supposed him to be saying that other people never really have what he has, but that, if he did say so, he would be talking nonsense; and he seemed quite definitely to reject the behaviourist view that "he has tooth-ache" means only that "he" is behaving in a particular manner; for he said that "tooth-ache" doesn't in fact only mean a particular kind of behaviour, and implied that when we pity a man for having toothache, we are not pitying him for putting his hand to his cheek; and, later on, he said that we *conclude* that another person has toothache from his behaviour, and that it is legitimate to conclude this on the analogy of the resemblance of his behaviour to the way in which we behave when we have toothache. It seemed, therefore, that just as to his first question he meant to give definitely the answer "No", so to his second question he meant to give definitely the answer "Yes; the word "toothache" is used in the same sense when we say that he has it (or "you have it") as when we say that I have it, though he never expressly said so; and though he seemed to throw some doubt on whether he meant this by saying "I admit that other people do have tooth-ache—this having the meaning which we have given it".

It seemed, therefore, that he did not think that the difference between "I have tooth-ache" and "He has tooth-ache" was due to the fact that the word "tooth-ache" was used in a different sense in the two sentences. What then was it due to? Much that he said seemed to suggest that his view was that the difference was due to the fact that in "He has toothache" we were necessarily talking of a physical body, whereas in "I have tooth-ache" we were not. As to the first of these two propositions he did not seem quite definite; for though at first he said that "my voice" means "the voice which comes from my mouth", he seemed afterwards to suggest that in "He has toothache" (or "You have") we were not necessarily referring to a *body*, but might be referring only to a *voice*, identified as "his" or "yours" without reference to a body. But as to the second proposition, the one about "I have tooth-ache", the point on which he seemed most anxious to insist was that what we call "having tooth-ache" is what he called a "primary experience" (he once used the phrase "direct experience" as equivalent to this one); and he said that "what characterizes 'primary experience'" is that in its case "'I' does not denote a possessor". In order to make clear what he meant by this he compared "I have tooth-ache" with "I see a red patch"; and said of what he called "visual sensations" generally, and in particular of what he called "the visual field", that "the idea of a person doesn't enter into the description of it, just as a [physical] eye doesn't enter into the description of what is seen"; and he said that similarly "the idea of a person" doesn't enter into the description of "having tooth-ache". How was he here using the word "person"? He certainly meant to deny that the idea of a physical body enters necessarily into the description; and in one passage he seemed to imply that he used "person" to mean the same as "physical body", since he said "A description of a sensation does not contain a description of a sense-organ, nor, *therefore*, of a person". He was, therefore, still maintaining apparently that one distinction between "I have toothache" and "He has toothache" was due to the fact that the latter necessarily refers to a physical body (or, perhaps, to a voice instead) whereas the former does not. But I think this was not the only distinction which he had in mind, and that he was not always using "person" to mean the same as physical body (or, perhaps, a voice instead). For he said that "Just as no [physical] eye is involved in seeing, so no Ego is involved in thinking or in having toothache"; and he quoted, with apparent approval, Lichtenberg's saying "Instead of 'I think' we ought

to say 'It thinks'" ("it" being used, as he said, as "Es" is used in "Es blitzet"); and by saying this he meant, I think, something similar to what he said of "the eye of the visual field" when he said that it is not anything which is *in* the visual field. Like so many other philosophers, in talking of "visual sensations" he seemed not to distinguish between "what I see" and "my seeing of it"; and he did not expressly discuss what appears to be a possibility, namely, that though no person enters into what I see, yet some "person" other than a physical body or a voice, may "enter into" my seeing of it.

In this connexion, that in "I have toothache" "I" does not "denote a possessor", he pointed out that, when I talk of "*my* body", the fact that the body in question is "mine" or "belongs to me", cannot be verified by reference to that body itself, thus seeming to imply that when I say "This body belongs to me", "me" is used in the second of the senses which he distinguished for "I", *viz.* that in which, according to him, it does not "denote a possessor". But he did not seem to be quite sure of this, since he said in one place "*If* there is an ownership such that I possess a body, this isn't verified by reference to a body", *i.e.* that "This is *my* body" can't possibly mean "This body belongs to this body". He said that, where "I" is replaceable by "this body" "I" and "he" are "on the same [grammatical] level". He was quite definite that the word "I" or "any other word which denotes a subject" is used in "two utterly different ways", one in which it is "on a level with other people", and one in which it is not. This difference, he said, was a difference in "the grammar of our ordinary language". As an instance of one of these two uses, he gave "I've got a match-box" and "I've got a bad tooth", which he said were "on a level" with "Skinner has a match-box" and "Skinner has a bad tooth". He said that in these two cases "I have . . ." and "Skinner has . . ." really were values of the same propositional function, and that "I" and "Skinner" were both "possessors". But in the case of "I have tooth-ache" or "I see a red patch" he held that the use of "I" is utterly different.

In speaking of these two senses of "I" he said, as what he called "a final thing", "In one sense 'I' and 'conscious' are equivalent, but not in another", and he compared this difference to the difference between what can be said of the pictures on a film in a magic lantern and of the picture on the screen; saying that the pictures in the lantern are all "on the same level" but that the picture which is at any given time on the screen is not "on the same level" with any of them, and that if we were to

use "conscious" to say of one of the pictures in the lantern that it was at that time being thrown on the screen, it would be meaningless to say of the picture on the screen that it was "conscious". The pictures on the film, he said, "have neighbours", whereas that on the screen has none. And he also compared the "grammatical" difference between the two different uses of "I" with the difference between the meaning of "has blurred edges" as applied to the visual field, and the meaning of the same expression as applied to any drawing you might make of the visual field: your drawing might be imagined to have sharp edges instead of blurred ones, but this is unimaginable in the case of the visual field. The visual field, he said, has no outline or boundary, and he equated this with "It has no sense to say that it has one".

In connexion with his statement that "I", in one of its uses, is equivalent to "conscious", he said something about Freud's use of the terms "conscious" and "unconscious". He said that Freud had really discovered phenomena and connexions not previously known, but that he talked as if he had found out that there were in the human mind "unconscious" hatreds, volitions, etc., and that this was very misleading, because we think of the difference between a "conscious" and an "unconscious" hatred as like that between a "seen" and an "unseen" chair. He said that, in fact, the grammar of "felt" and "unfelt" hatred is quite different from that of "seen" and "unseen" chair, just as the grammar of "artificial" flower is quite different from that of "blue" flower. He suggested that "unconscious toothache", if "unconscious" were used as Freud used it, might be necessarily bound up with a physical body, whereas "conscious toothache" is not so bound up.

As regards Solipsism and Idealism he said that he himself had been often tempted to say "All that is real is the experience of the present moment" or "All that is certain is the experience of the present moment"; and that any one who is at all tempted to hold Idealism or Solipsism knows the temptation to say "The only reality is the present experience" or "The only reality is *my* present experience". Of these two latter statements he said that both were equally absurd, but that, though both were fallacious, "the idea expressed by them is of enormous importance". Both about Solipsism and about Idealism he had insisted earlier that neither of them pretends that what it says is learnt by experience—that the arguments for both are of the form "you can't" or "you must", and that both these expressions "cut [the statement in question] out of our language".

Elsewhere he said that both Solipsists and Idealists would say they "couldn't imagine it otherwise", and that, in reply to this, he would say, "If so, your statement has no sense" since "nothing can characterize reality, except as opposed to something else which is not the case". Elsewhere he had said that the Solipsist's statement "Only my experience is real" is absurd "as a statement of fact", but that the Solipsist sees that a person who says "No : my experience is real too" has not really refuted him, just as Dr. Johnson did not refute Berkeley by kicking a stone. Much later he said that Solipsism is right if it merely says that "I have tooth-ache" and "He has tooth-ache" are "on quite a different level", but that "if the Solipsist says that he has something which another hasn't, he is absurd and is making the very mistake of putting the two statements on the same level". In this connexion he said that he thought that both the Realist and the Idealist were "talking nonsense" in the particular sense in which "nonsense is produced by trying to express by the use of language what ought to be embodied in the grammar"; and he illustrated this sense by saying that "I can't feel his toothache" means "'I feel his toothache' has no sense" and therefore does not "express a fact" as "I can't play chess" may do.

(E) He concluded (III) by a long discussion which he introduced by saying "I have always wanted to say something about the grammar of ethical expressions, or, e.g. of the word 'God'". But in fact he said very little about the grammar of such words as "God", and very little also about that of ethical expressions. What he did deal with at length was not Ethics but Aesthetics, saying, however, "Practically everything which I say about 'beautiful' applies in a slightly different way to 'good'". His discussion of Aesthetics, however, was mingled in a curious way with criticism of assumptions which he said were constantly made by Frazer in the "Golden Bough", and also with criticism of Freud.

About "God" his main point seemed to be that this word is used in many *grammatically* different senses. He said, for instance, that many controversies about God could be settled by saying "I'm not using the word in such a sense that you can say . . .", and that different religions "treat things as making sense which others treat as nonsense, and don't merely deny some proposition which another religion affirms"; and he illustrated this by saying that if people use "god" to mean something like a human being, then "God has four arms" and

"God has two arms" will both have sense, but that others so use "God" that "God has arms" is nonsense—would say "God can't have arms". Similarly, he said of the expression "the soul", that sometimes people so use that expression that "the soul is a gaseous human being" has sense, but sometimes so that it has not. To explain what he meant by "grammatically" different senses, he said we wanted terms which are not "comparable", as e.g. "solid" and "gaseous" are comparable, but which differ as, e.g. "chair" differs from "permission to sit on a chair", or "railway" from "railway accident".

He introduced his whole discussion of Aesthetics by dealing with one problem about the meaning of words, with which he said he had not yet dealt. He illustrated this problem by the example of the word "game", with regard to which he said both (1) that, even if there is something common to all games, it doesn't follow that this is what we mean by calling a particular game a "game", and (2) that the reason why we call so many different activities "games" need not be that there is anything common to them all, but only that there is "a gradual transition" from one use to another, although there may be nothing in common between the two ends of the series. And he seemed to hold definitely that there is nothing in common in our different uses of the word "beautiful", saying that we use it "in a hundred different games"—that, e.g. the beauty of a face is something different from the beauty of a chair or a flower or the binding of a book. And of the word "good" he said similarly that each different way in which one person, A, can convince another, B, that so-and-so is "good" fixes the meaning in which "good" is used in that discussion—"fixes the grammar of that discussion"; but that there will be "gradual transitions", from one of these meanings to another, "which take the place of something in common". In the case of "beauty" he said that a difference of meaning is shown by the fact that "you can say more" in discussing whether the arrangement of flowers in a bed is "beautiful" than in discussing whether the smell of lilac is so.

He went on to say that specific colours in a certain spatial arrangement are not merely "symptoms" that what has them also possesses a quality which we call "being beautiful", as they would be, if we meant by "beautiful", e.g. "causing stomach-ache", in which case we could learn by experience whether such an arrangement did always cause stomach-ache or not. In order to discover how we use the word "beautiful" we need, he said, to consider (1) what an actual aesthetic controversy or

enquiry is like, and (2) whether such enquiries are in fact psychological enquiries "though they look so very different". And on (1) he said that the actual word "beautiful" is hardly ever used in aesthetic controversies : that we are more apt to use "right", as, e.g. in "That doesn't look quite right yet", or when we say of a proposed accompaniment to a song "That won't do : it isn't right". And on (2) he said that if we say, e.g. of a bass "It is too heavy ; it moves too much", we are not saying "If it moved less, it would be more agreeable to me" : that, on the contrary, that it should be quieter is an "end in itself", not a means to some other end ; and that when we discuss whether a bass "will do", we are no more discussing a psychological question than we are discussing psychological questions in Physics ; that what we are trying to do is to bring the bass "nearer to an ideal", though we haven't an ideal before us which we are trying to copy ; that in order to show what we want, we might point to another tune, which we might say is "perfectly right". He said that in aesthetic investigations "the one thing we are not interested in is causal connexions, whereas this is the only thing we are interested in in Psychology". To ask "Why is this beautiful?" is not to ask for a causal explanation : that, e.g. to give a causal explanation in answer to the question "Why is the smell of a rose pleasant?" would not remove our "aesthetic puzzlement".

Against the particular view that "beautiful" means "agreeable" he pointed out that we may refuse to go to a performance of a particular work on such a ground as "I can't stand its greatness", in which case it is disagreeable rather than agreeable ; that we may think that a piece of music which we in fact prefer is "just nothing" in comparison to another to which we prefer it ; and that the fact that we go to see "King Lear" by no means proves that that experience is agreeable : he said that, even if it is agreeable, that fact "is about the least important thing you can say about it". He said that such a statement as "That bass moves too much" is not a statement about human beings at all, but is more like a piece of Mathematics ; and that, if I say of a face which I draw "It smiles too much", this says that it could be brought closer to some "ideal", not that it is not yet agreeable enough, and that to bring it closer to the "ideal" in question would be more like "solving a mathematical problem". Similarly, he said, when a painter tries to improve his picture, he is not making a psychological experiment on himself, and that to say of a door "It is top-heavy" is to say what is wrong with it, *not* what impression

it gives you. The question of Aesthetics, he said, was not "Do you like this?" but "Why do you like it?"

What Aesthetics tries to do, he said, is to give *reasons*, e.g. for having this word rather than that in a particular place in a poem, or for having this musical phrase rather than that in a particular place in a piece of music. Brahms' *reason* for rejecting Joachim's suggestion that his Fourth Symphony should be opened by two chords was not that that wouldn't produce the feeling he wanted to produce, but something more like "That isn't what I meant". *Reasons*, he said, in Aesthetics, are "of the nature of further descriptions": e.g. you can make a person see what Brahms was driving at by showing him lots of different pieces by Brahms, or by comparing him with a contemporary author; and all that Aesthetics does is "to draw your attention to a thing", to "place things side by side". He said that if, by giving "reasons" of this sort, you make another person "see what you see" but it still "doesn't appeal to him", that is "an end" of the discussion; and that what he, Wittgenstein, had "at the back of his mind" was "the idea that aesthetic discussions were like discussions in a court of law", where you try to "clear up the circumstances" of the action which is being tried, hoping that in the end what you say will "appeal to the judge". And he said that the same sort of "reasons" were given, not only in Ethics, but also in Philosophy.

As regards Frazer's "Golden Bough", the chief points on which he seemed to wish to insist were, I think, the three following. (1) That it was a mistake to suppose that there was *only one* "reason", in the sense of "motive", which led people to perform a particular action—to suppose that there was "one motive, which was *the* motive". He gave as an instance of this sort of mistake Frazer's statement, in speaking of Magic, that when primitive people stab an effigy of a particular person, they believe that they have hurt the person in question. He said that primitive people do not *always* entertain this "false scientific belief", though in some cases they may: that they may have quite different reasons for stabbing the effigy. But he said that the tendency to suppose that there is "one motive which is *the* motive" was "enormously strong", giving as an instance that there are theories of play each of which gives *only one* answer to the question "Why do children play?" (2) That it was a mistake to suppose that *the* motive is always "to get something useful". He gave as an instance of this mistake Frazer's supposition that "people at a certain stage thought it useful to kill a person, in order to get a good crop". (3) That it was a

mistake to suppose that why, e.g. the account of the Beltane Festival "impresses us so much" is because it has "developed from a festival in which a real man was burnt". He accused Frazer of thinking that this was the reason. He said that our puzzlement as to why it impresses us is not diminished by giving the *causes* from which the festival arose, but is diminished by finding other similar festivals: to find these may make it seem "natural", whereas to give the causes from which it arose cannot do this. In this respect he said that the question "Why does this impress us?" is like the aesthetic questions "Why is this beautiful?" or "Why will this bass not do?"

He said that Darwin, in his "expression of the Emotions", made a mistake similar to Frazer's, e.g. in thinking that "because our ancestors, when angry, wanted to bite" is a sufficient explanation of why we show our teeth when angry. He said you might say that what is satisfactory in Darwin is not such "hypotheses", but his "putting the facts in a system"—helping us to make a "synopsis" of them.

As for Freud, he gave the greater part of two lectures to Freud's investigation of the nature of a "joke" (Witz), which he said was an "aesthetic investigation". He said that Freud's book on this subject was a very good book for looking for philosophical mistakes, and that the same was true of his writings in general, because there are so many cases in which one can ask how far what he says is a "hypothesis" and how far merely a good way of representing a fact—a question as to which he said Freud himself is constantly unclear. He said, for instance, that Freud encouraged a confusion between getting to know the *cause* of your laughter and getting to know the *reason* why you laugh, because what he says sounds as if it were science, when in fact it is only a "wonderful representation". This last point he also expressed by saying "It is all excellent similes, e.g. the comparison of a dream to a rebus". (He had said earlier that all Aesthetics is of the nature of "giving a good simile".) He said that this confusion between *cause* and *reason* had led to the disciples of Freud making "an abominable mess": that Freud did not in fact give any method of analysing dreams which was analogous to the rules which will tell you what are the causes of stomach-ache; that he had genius and therefore might sometimes by psycho-analysis find the *reason* of a certain dream, but that what is most striking about him is "the enormous field of psychical facts which he arranges".

As for what Freud says about jokes, he said first that Freud makes the two mistakes (1) of supposing that there is something

common to all jokes, and (2) of supposing that this supposed common character is the meaning of "joke". He said it is not true, as Freud supposed, that *all* jokes enable you to do covertly what it would not be seemly to do openly, but that "joke", like "proposition", "has a rainbow of meanings". But I think the point on which he was most anxious to insist was perhaps that psycho-analysis does not enable you to discover the *cause* but only the *reason* of, e.g. laughter. In support of this statement he asserted that a psycho-analysis is successful only if the patient agrees to the explanation offered by the analyst. He said there is nothing analogous to this in Physics; and that what a patient agrees to can't be a *hypothesis* as to the *cause* of his laughter, but only that so-and-so was the *reason* why he laughed. He explained that the patient who agrees did not think of this reason at the moment when he laughed, and that to say that he thought of it "subconsciously" "tells you nothing as to what was happening at the moment when he laughed".

(F) In (I), rather to my surprise, he spent a good deal of time in discussing what would usually be called a question about colours, namely, the question how the four "saturated" colours, pure yellow, pure red, pure blue and pure green, which he called "primary", are distinguished from those "saturated" colours which are not "primary". He drew a circle on the blackboard to represent the arrangement of the saturated colours, with a vertical diameter joining "yellow" at the top to "blue" at the bottom, and a horizontal diameter joining "green" on the left to "red" on the right. And he seemed to be maintaining with regard to these four colours that they are distinguished from the other saturated colours in the two following ways, *viz.* (1) that the sense in which any purple is "between" pure red and pure blue, and in which any orange is "between" pure yellow and pure red is very different from the sense of "between" in which pure red is "between" any orange and any purple; a difference which he also expressed by saying that whereas an orange can be properly called a "mixture" of yellow and red, red cannot possibly be called a "mixture" of orange and purple; and (2) that whereas pure red can be properly said to be "mid-way" between pure yellow and pure blue, there is no colour which is "midway" between pure red and pure blue, or "mid-way" between pure yellow and pure red, etc. He said that, for these reasons, the arrangement of the saturated colours in a square, with the four "primaries" at the four corners, is a better picture of their relations than the arrangement of them in a circle.

I say only that he *seemed* to be making these assertions, because he emphasized from the beginning that "primary" is not an adjective to "colour" in the sense in which "black" may be an adjective to "gown", but that the distinction between "primary" and "not primary" is a "logical" distinction—an expression which he explained later on by saying that, just as sounds are not distinguished from colours by the fact that something is true of the one which is not true of the other, so red, blue, green and yellow are not distinguished from the other saturated colours by the fact that anything is true of them which is not true of the others. He emphasized to begin with that the sentences "blue is not primary" and "violet is primary" are both of them "nonsense", and I think there is no doubt he held that, since this is so, their contradictories "blue is primary" and "violet is not primary" are also nonsense, though there is a sense in which the two last are true, and the two former false. In other words, I think he certainly held that "blue is primary" is a "necessary proposition"—that we can't imagine its not being true—and that therefore, as he said (p. 16), it "has no sense". It would seem to follow that if, as he seemed to be, he was really talking about the *colours*, red, blue, green and yellow, all that he said about them was "nonsense". According to what he said elsewhere, he could only have been talking sense, if he was talking, not about the colours, but about certain words used to express them; and accordingly he did actually go on to say that "red is primary" was only a proposition about the use of the English word "red", which, as I said (p. 311), he cannot seriously have held. The question I am here raising is the question which I discussed at length in my second article, and I have nothing to add except to give one quotation which I ought to have given there. He actually said, in one place in (II), "What corresponds to a necessity in the world must be what in language seems an arbitrary rule". I do not think he had succeeded in getting quite clear as to what relation he wished to assert to hold between what he called "rules of grammar", on the one hand, and "necessary propositions", on the other.

(G) With questions about Time he dealt, at considerable length, in two places in (III).

The earlier discussion was in connexion with his view that the "troubles in our thought" which he was concerned to remove, arise from our thinking that sentences which we do not use with any practical object, sound as if they "ought to have sense",

when in fact they have none. And in this connexion his main point seemed to be that, since we talk of Time "flowing" as well as of a river "flowing", we are tempted to think that Time "flows" in a certain "direction", as a river does, and that therefore it has sense to suppose that Time might flow in the opposite direction, just as it certainly has sense to suppose that a river might. He said, in one place, that some philosophers have actually made the muddle of thinking that Time has a "direction" which might conceivably be reversed. Later on he made a distinction, as to the meaning of which I am not clear, between what he called "memory-time" and what he called "information-time", saying that in the former there is only earlier and later, not past and future, and that it has sense to say that I remember that which in "information-time" is future. This distinction seemed to be connected with one he had made earlier, when he said that, if we imagine a river with logs floating down it at equal spatial distances from one another, the interval between the time at which, e.g. the 120th log passed us and that at which, e.g. the 130th passed, might seem to be equal to that between the time at which the 130th passed us and that at which the 140th passed us, although, measured by a clock, these intervals were not equal. He went on to ask: "Supposing all events had come to an end, what is the criterion for saying that Time would have come to an end too, or that it still went on?" and to ask: "If there were no events earlier than a hundred years ago, would there have been no time before that?" He said that what we need to do is to notice how we use the expression "Time"; and that people ask "Has Time been created?" although the question "Has 'before' been created?" has absolutely no meaning.

But he said a good many things in this discussion which I have failed to understand, and I may easily have omitted points which he would have considered of the first importance.

In his second discussion he was trying to show what was wrong with the following statement which Russell made in his "Outline of Philosophy": "Remembering, which occurs now, can not possibly prove that what is remembered occurred at some other time, because the world might have sprung into being five minutes ago, full of acts of remembering which were entirely misleading." But I cannot help thinking that, in what he said about this statement, he made two quite definite mistakes as to what Russell was implying by it. In order to explain why I think so I must, however, first explain what I take it that Russell was implying.

It will be noted that Russell speaks as if "acts of remembering" could be "entirely misleading"; and he seems not to have noticed that we so use the term "remember" that if an act, which resembles an act of remembering, turns out to be entirely misleading, we say that it was not an act of remembering. For instance "I remember that I had breakfast this morning" is so used that, if it turns out that I did not have breakfast this morning, it follows logically that I do not remember that I did: from "I remember that I had it" it follows logically that I did have it, so that "acts of remembering, which are entirely misleading" is a contradiction in terms; if an act is entirely misleading, it is not an act of remembering. It is plain, therefore, that Russell was using the expression "acts of remembering" in a different sense from any in which it can be correctly used; and his view could be more correctly expressed as the view that it is *logically possible* that we never remember anything. I say "*logically possible*", because when he says "the world *might* have sprung into being five minutes ago", I think he certainly means by "*might*", merely that it is *logically possible* that it did.

Now Wittgenstein pointed out, quite justly, that when Russell says "The world might have sprung into being five minutes ago" his choice of "five minutes ago" as the time when the world might have "sprung into being" is "arbitrary": Russell's view requires that it is equally true that it might have "sprung into being" two minutes ago or one minute ago, or, says Wittgenstein, that it might have begun to exist *now*: he actually said that Russell *ought* to have said "The world might have been created *now*". And I think it is true that Russell does imply this. But Wittgenstein said that in the statement quoted, Russell was "committing the precise fallacy of Idealism". And surely this is a complete mistake! From what I have quoted (p. 15) it appears clear that what Wittgenstein regarded as the "fallacy of Idealism" was some such statement as "It is logically impossible that anything should be real except the present experience". And Russell's statement certainly does not imply this. It looks to me as if, for the moment, Wittgenstein was confusing the two entirely different propositions, (1) "It is logically possible that nothing exists except the present experience" which Russell may be said to imply, and (2) "It is logically impossible that anything should exist except the present experience", which he certainly does not imply.

But it seems to me that he also made another complete mistake as to what Russell's view implied; and this was a criticism into which he went at some length. He began by asking us to

consider the question "What is the verification for the proposition 'The world began to exist five minutes ago'?" saying that, if you admit no criterion for its truth, that sentence is "useless", or, as he afterwards said, "meaningless". And his criticism of Russell here consisted in saying that "Russell is refusing to admit as evidence for 'the world began more than five minutes ago' what we all admit as such evidence, and is therefore making that statement meaningless". He compared Russell's statement to the statement "There is a rabbit between A and B, whenever nobody is looking" which he said "seems to have sense, but is in fact meaningless, because it cannot be refuted by experience". But surely Russell would admit and can perfectly consistently admit, that some of those events, which he calls incorrectly "acts of remembering" do constitute very strong evidence that the world existed more than five minutes ago. He is not concerned to deny that they constitute *strong* evidence, but only that they constitute *absolutely conclusive* evidence—that they "prove" that it did. In other words, he is only asserting that it is *logically possible* that the world did not. Wittgenstein seems to me to have overlooked the distinction between denying that we have *any* evidence which Russell does not do, and denying that we have *absolutely conclusive* evidence, which I think Russell certainly meant to do.

But later on Wittgenstein seemed to me to be suggesting another quite different argument, which, if he did mean what he seemed to mean, and if what he seemed to mean is true, would really be a valid refutation of Russell's statement. He introduced again the phrase "memory-time", saying that a certain order of events might be so called, and then going on to say that all these events "approach a point such that it will have no sense to say 'B occurred after the present in memory-time'"; that "now" "should be a point in an order"; and that when we say "The clock is striking now", "now" means "the present of our memory-time", and cannot mean, e.g. "at 6.7" because it has sense to say "It is 6.7 now". I think all this suggests that his view was that "now", in the sense in which we commonly use it, and in which Russell was undoubtedly using it, has a meaning such that part of what we are saying when we say that an event is happening "now", is that it was preceded by other events which we remember; and, if this is true, it would certainly follow that Russell was wrong in implying that it is logically possible that nothing should have happened before *now*.

(H) I was a good deal surprised by some of the things he said about the difference between "philosophy" in the sense in which what he was doing might be called "philosophy" (he called this "modern philosophy"), and what has traditionally been called "philosophy". He said that what he was doing was a "new subject", and not merely a stage in a "continuous development"; that there was now, in philosophy, a "kink" in the "development of human thought", comparable to that which occurred when Galileo and his contemporaries invented dynamics; that a "new method" had been discovered, as had happened when "chemistry was developed out of alchemy"; and that it was now possible for the first time that there should be "skilful" philosophers, though of course there had in the past been "great" philosophers.

He went on to say that, though philosophy had now been "reduced to a matter of skill", yet this skill, like other skills, is very difficult to acquire. One difficulty was that it required a "sort of thinking" to which we are not accustomed and to which we have not been trained—a sort of thinking very different from what is required in the sciences. And he said that the required skill could not be acquired merely by hearing lectures: discussion was essential. As regards his own work, he said it did not matter whether his results were true or not: what mattered was that "a method had been found".

In answer to the question why this "new subject" should be called "philosophy" he said in (III) that though what he was doing was certainly different from what, e.g. Plato or Berkeley had done, yet people might feel that it "takes the place of" what they had done—might be inclined to say "This is what I really wanted" and to identify it with what they had done, though it is really different, just as (as I said above, p. 9) a person who had been trying to trisect an angle by rule and compasses might, when shown the proof that this is impossible, be inclined to say that this impossible thing was the very thing he had been trying to do, though what he had been trying to do was really different. But in (II) he had also said that the "new subject" did really resemble what had been traditionally called "philosophy" in the three respects that (1) it was very general, (2) it was fundamental both to ordinary life and to the sciences, and (3) it was independent of any special results of science; that therefore the application to it of the word "philosophy" was not purely arbitrary.

He did not expressly try to tell us exactly what the "new method" which had been found was. But he gave some hints

as to its nature. He said, in (II), that the "new subject" consisted in "something like putting in order our notions as to what can be said about the world", and compared this to the tidying up of a room where you have to move the same object several times before you can get the room really tidy. He said also that we were "in a muddle about things", which we had to try to clear up; that we had to follow a certain instinct which leads us to ask certain questions, though we don't even understand what these questions mean; that our asking them results from "a vague mental uneasiness", like that which leads children to ask "Why?"; and that this uneasiness can only be cured "either by showing that a particular question is not permitted, or by answering it". He also said that he was not trying to teach us any new facts: that he would only tell us "trivial" things—"things which we all know already"; but that the difficult thing was to get a "synopsis" of these trivialities, and that our "intellectual discomfort" can only be removed by a synopsis of *many* trivialities—that "if we leave out any, we still have the feeling that something is wrong". In this connexion he said it was misleading to say that what we wanted was an "analysis", since in science to "analyse" water means to discover some new fact about it, e.g. that it is composed of oxygen and hydrogen, whereas in philosophy "we know at the start all the facts we need to know". I imagine that it was in this respect of needing a "synopsis" of trivialities that he thought that philosophy was similar to Ethics and Aesthetics (p. 19).

I ought, perhaps, finally to repeat what I said in my first article (pp. 5-6), namely, that he held that though the "new subject" must say a great deal about language, it was only necessary for it to deal with those points about language which have led, or are likely to lead, to definite philosophical puzzles or errors. I think he certainly thought that some philosophers now-a-days have been misled into dealing with linguistic points which have no such bearing, and the discussion of which therefore, in his view, forms no part of the proper business of a philosopher.

II.—THE PROBLEM OF PERCEPTION

By A. M. QUINTON

I

THE problem of perception is to give an account of the relationship of sense-experience to material objects. This relationship has traditionally been seen as logical, a matter of showing how beliefs about objects can be established or supported by what we know in immediate experience. For, it is held, only our knowledge of experience is direct, immediate, by acquaintance; what we know or claim to know about objects is indirect, derivative, by inference from what we know directly. Consequently if our beliefs about objects are to have any secure foundation, it must consist in what we know directly, by acquaintance, about sense-data. From this starting-point philosophers have gone on to present varying accounts of the type of inference involved. An extreme view is Hume's, that the passage from experiences to objects rests on 'a kind of fallacy or illusion'. Lockean causal theories assert that the connexion between experiences and objects is contingent and that knowledge of experience is good inductive evidence for beliefs, logically distinct from it about objects. The species of inference involved is transcendental hypothesis of the type to be found in scientific arguments for the existence of such unobservables as electrons or chromosomes. For phenomenism the connexion between experiences and objects is necessary, to speak of objects is to speak in an abbreviated way about certain pervasive kinds of regularity in experience. The species of inference involved is simple inductive extrapolation. There are not two worlds, an inner and an outer, but two terminologies. The terminology of objects is used to refer to what is invariant as between the private worlds of experience.

Each view derives strength from the weaknesses of its opponent. The most emphasised weakness of phenomenism is that, if it were true, unobserved objects would be mere possibilities and actual effects would have to arise from merely potential causes. Mill's view that objects are permanent possibilities of sensation is confronted by a fundamental and unargued incredulity. A more serious difficulty arises about the antecedents of the

hypothetical statements which describe the permanent possibilities in question. For these antecedents mention objects. To assume, as phenomenalists often cheerfully do, that these references can be replaced by references to 'orienting experiences' is to beg the very question at issue. One cannot *assume* that statements about experiences are equivalent in meaning to statements about objects in order to *show* that they are. Against the causal theory it is argued that, given the sense-datum theory, it would be impossible ever to know that the logically distinct, unobservable, transcendental causes existed. For a causal inference is only legitimate if it is at least possible to obtain evidence for the existence of the cause which is independent of the events it is held to explain.

In the face of this impasse sense-datum theorists have tended to adopt a middle position of compromise. Causal theorists liken their procedure to the 'model-building' of natural scientists. The external world is a theoretical construction, fruitful and various in its predictive and explanatory consequences. Phenomenalists modify their thesis of the strict logical equivalence of statements about experiences and about objects, in view of the difficulties, in principle and practice, of translating one into the other. Both extremes are abandoned in favour of the view that it is a simple, convenient and fruitful theoretical construction. But this is rather a method of refusing to face the difficulties than of overcoming them. For what sort of theoretical construction is involved, a substantial model of the not-yet-observed like a theory of atomic structure or a mere *façon de parler* like theories of magnetic and gravitational fields?

My purpose in this paper is to overcome these difficulties by a more radical procedure, that of refuting the premise from which both problematic doctrines derive, that we are never directly aware of or acquainted with objects.

My principal target will be the conception of direct awareness or acquaintance itself. The sense-datum theory holds that corresponding to the two kinds of objects of knowledge are two kinds of knowledge—direct and indirect. Thus while no knowledge of material objects is direct, all or only knowledge of experience is direct. In more linguistic terms, while no statements about objects are basic, all or only statements about experience are basic. A piece of knowledge, then, is direct if, and only if, it can be expressed by a basic statement. But this translation is of little help since neither of the crucial terms, 'direct' and 'basic', is clearly intelligible, let alone more intelligible than the other.

Two main kinds of definition are commonly offered of these expressions, one in terms of certainty, the other in terms of inference. By the former I directly know that p (or ' p ' is a basic statement) if I know for certain that p . It is held that beliefs about objects are never certain, beliefs about experience are always certain and that for any uncertain belief to be even probable something else must be certain. Consequently all beliefs about objects that are to any extent probable must be logically derived from beliefs about experience. I shall hold that all three of the premises for this conclusion are false. The incorrigibility of statements about experience has been defended, notably by Ayer, on the ground that the only mistakes to which we are liable in making such statements are 'verbal'. I shall attempt to show that this too is false. Sometimes a definition in terms of inference is preferred. I directly know that p (or ' p ' is a basic statement) if I know that p without inference. It is not, of course, maintained that in coming to form a belief about an object I undertake any conscious process of reasoning. What is involved is 'implicit' inference. Nevertheless, it is held, reasons exist for beliefs about objects which it is the philosopher's business to render explicit and without reference to which no justification of these beliefs can be provided. I shall argue that there is no relevant sense of 'reason' in which a reason for them always exists.

Why should this have been thought to be so? The sense-datum theory, seemingly a variant of the empiricist principle that all our knowledge of matters of fact is based on sense-experience, tends to assume that principle's authority. But this, like other oracles, owes much of its reputation to ambiguity. It can be taken to assert three different things, two of which are uncontentious while the third deserves close inspection. First, it is an unexciting truth of physiology that sensations, physical stimulations of the sense-organs, are causally necessary conditions of our knowledge of matters of fact. Second, the establishment of any truth about objects logically requires that someone shall have seen, touched or otherwise perceived something. The chains of inference and testimony cannot hang unsupported but must terminate in observation. In this use 'sense-experience' does not mean anything so definite as 'sense-datum', it has no phenomenological flavour. Seeing a tomato is just as much an observation as seeing a round, red, shiny patch. Finally, 'based on sense-experience' can be taken to mean 'logically derived from sense-experience'. The logical derivation in question here is of statements about objects from

statements about experiences. It is this third interpretation of the principle that constitutes the sense-datum theory and which I shall attempt to refute.

These definitions of 'direct' and 'basic' in terms of certainty and inference are not, however, the starting-points of sense-datum theories of perception. They are rather conclusions to the argument from illusion in terms of which the expressions 'direct' and 'basic' are normally introduced. This argument holds that objects are not always what they appear to be and that there need be no discoverable difference between two situations in one of which an object is and in the other is not what it appears to be. In consequence, all that we really know is what appears to be the case, since, even when what appears to be the case *is* the case, we cannot there and then tell whether it is or not. Since we know only what appears to be the case, the only things we really perceive are appearances. Some philosophers have protested weakly against the later stages of this argument. I hope to substantiate and fortify their protest.

The mistake lies in the identification of what appears to be the case with our sense-experience. We always know what appears to be the case. So it is appearances, not objects, that we really perceive. But what else are these appearances but our current sense-fields, our sense-experience? The three forms of words; 'this appears to be ϕ ', 'there is a ϕ appearance', 'there is a ϕ sense-datum', are held to be equivalent in meaning. I shall argue that a statement of what appears to be the case is rarely a description of our sense-experience and is normally a modified, guarded claim about what *is* the case, expressing an inclination to believe something about objects. The ostensible firmness and incorrigibility of these assertions is a consequence, not of their referring to a class of private, given entities, but rather of the modesty of the claim they make. So what the argument from illusion establishes is not that we always infallibly know what our sense-experience is like, but only that, whether or not we *know* what is the case, we can always say, without much fear of contradiction, what we are inclined to *believe* is the case. These statements do not, then, express a special kind of direct knowledge by acquaintance nor are they premises from which statements about objects could be inferred. For they are not claims to knowledge at all, but more or less tentative expressions of belief, and what is tentatively affirmed is precisely the same as, and thus cannot be a premise for, what, in the conclusion of the supposed inference, we claim to know without hesitation. I shall argue, however, that we can, and rather infrequently do,

describe our experience and that we can do this in statements containing such expressions as 'look', 'appear' and 'seem'.

The consequences of this distinction of 'appearances' from sense-data are that knowledge about experience is much less common than is widely supposed and that the greater part of our 'knowledge of appearances' is not capable of figuring as premises in inferences to beliefs about objects.

Before embarking on this another familiar argument for the sense-datum theory must be considered : what may be called the argument from scientific knowledge. There is conclusive evidence for the fact that many of our sense-experiences occur appreciably later than the events of which they give us knowledge, in particular the experiences caused by what is astronomically visible or less remotely audible. More generally, every sense-experience is at the end of a temporally extended causal chain whose first member is the supposedly perceived occurrence. Consequently, what we directly perceive, the object of acquaintance, cannot be the same as that about which we claim knowledge. But this involves no new issue of principle. It shows objects and experiences to be temporally distinct where the argument from illusion shows them to be much more generally different in character. It only shows that we do not directly perceive objects if the supposed consequence of the argument from illusion—that we perceive only our sense-experience directly—is already accepted.

The view common to all versions of the sense-datum theory that the perception of objects is really a kind of inference seems to arise from a belief that, while perception proper must be infallible, inference need not be, and thus that all mistakes are fallacies. But both perception and inference are learnt, intelligent activities which we can presumably perform with varying degrees of efficiency and success. That perception is an acquired skill has perhaps been an inducement to regard it as inference to those who suppose all intelligent activities to be species of reasoning.

Ultimately the problem of perception is that of the relation of thought or language to the world. There is a distressing correspondence with primitive cosmology. Some statements are supported by others, but what supports these others, what is tortoise to their elephant ? For the whole system of knowledge cannot support itself in mid-air ; it is not self-contained. There is a dilemma here. Either the ultimate support is logically related to the body of knowledge and is thus automatically brought inside the body of knowledge, since only

statements can stand in logical relations, and, if so, the question of dependence on the extralinguistic world breaks out again. Or it is not and there is no answer in terms of correct inference to the request for a justification of reliance on this ultimate support.

Philosophers have sought to evade this dilemma by recourse to the Janus-faced notion of experience. The fact that we cannot, it seems, have an experience without somehow being conscious or aware of it has seemed to provide foundation-stones for the edifice of knowledge which are at once statements, capable of standing in logical relations to the rest of the structure, and parts, perhaps the sole constituents, of the extralinguistic world, self-describing entities. I shall contend that there are no such things and opt for the second horn of the dilemma which, as I hope to show, is a less painful resting-place than it might seem.]

II

Our first problem is to evaluate the argument from illusion. From the unexceptionable premises that things are not always what they appear to be and that we cannot always tell, there and then, whether they are or not, it is concluded that we have direct knowledge only of appearances, never of objects. For there need be no immediately discoverable difference between two appearances of which one is in fact 'veridical' and the other 'delusive'. So what we really perceive are appearances, whether they are veridical or not depends on something that lies outside the perceptual situation. But what are these appearances that we perceive? They are, it is said, sense-data, the given, immediate experience, they are the current states of our sense-fields.

Of some uses of 'appear', 'seem', etc. it is clearly untrue to say that they figure in descriptions of experience. 'They appear to be away', said when the twice-rung doorbell of a house with drawn curtains remains unanswered, means much the same as 'they must be away' or 'they are probably away'. We are not here describing, but drawing conclusions from, what we observe. The word 'appear' serves to indicate that these conclusions are drawn with less than full confidence. There is nothing 'basic' about them.

But there is another use of 'appear' in which no reason can be given for statements containing it and which do report observations. 'It appears to be green' we might say of a distant house. If challenged we can only repeat, or perhaps correct, ourselves

or protest, 'well, that is how it appears to me'. But such a statement would normally be made in answer to such questions as 'what colour is that house' and could be replaced by 'it's green, I think' or 'it's green, isn't it?' They report observations in a tentative way where we know, believe or suspect that the circumstances are unfavourable to an accurate report, that there is something wrong with or abnormal about the conditions of observation. They resemble ordinary categorical descriptions, 'that house is green', in subject-matter, but differ from them in expressing inclinations to believe rather than full beliefs.

There is a third use of 'appear', which resembles the one last mentioned, in that no reasons or evidence can be given for statements containing it, but differs from it in that certain conventional conditions of observation are supposed to obtain, whether they do or not. 'It looks to me (here, now) elliptical' we say of a plate we know to be tilted and round, supposing it to be at right angles to our line of vision. This statement answers the question 'how does it strike you, look to you, what exactly do you see?' It is replaceable by 'there is an elliptical patch in the centre of my visual field'. It is in this type of case only that the description of appearances and experience coincide.

Consider that old friend the stick half in, half out, of water. One might say of it (a) 'it is straight', (b) 'it looks bent but is really straight', (c) 'it looks bent', (d) 'it is bent'. Statement (a) is true, (b) describes the stick correctly and points out how one might be led to make a mistake about it if unaware of an abnormality (a refracting medium) in the conditions of observation, (c) gives tentative expression to the inclination mistakenly to believe (d) which is straightforwardly false. 'It looks bent' is the puzzling case. For it may be a guarded way of saying 'it is bent' (denied by 'it isn't bent') or a way of saying 'most people would be inclined to say it was bent' (denied by 'it doesn't') or a way of saying 'it looks bent to me, here, now' (which can only be denied by 'oh surely not').

So, even when not used to give tentative conclusions from evidence, the verb 'appear' and its cognates are seldom used to describe experience, but primarily to give tentative descriptions of objects. In other words, the 'appearances' that survive the argument from illusion as the proper objects of acquaintance are not ordinarily sense-experiences. These seemingly rock-bottom matters of fact are, in a way, incorrigible and, *ex hypothesi*, uninferred. But their incorrigibility is imperfect and spurious. Imperfect because both 'this is ϕ , I think' and 'this is ϕ , most people would say' can be contradicted (by 'it isn't' and 'they

wouldn't') and revised accordingly. Spurious because it arises, not from their making a definite claim about something private, but from their making a weak, indefinite claim about something public. And, though uninferrable, they cannot play the part of premises in inferences to categorical descriptions of objects. 'This appears to be ϕ ' is no more evidence or a reason for 'this is ϕ ' than are 'this may be ϕ ' or 'this is probably ϕ '. All three are simply modified ways of saying 'this is ϕ ', appropriate for one who is inclined, but not inclined quite confidently enough, to make the categorical statement itself.

This is not to deny that we can and do describe our experience. All I have tried to show is that we describe it very much less often than is usually supposed. Being unsure about the circumstances is a common enough occurrence. But the description of experience proper is a sophisticated procedure and one seldom called for. It is an essential accomplishment for painters, broadcasting engineers, doctors of the eye and ear, cooks and experimental psychologists. But unless we fall into their hands there is little need for us to become proficient in it. The sophistication arises with the deliberate supposition that conditions obtain which we have no reason to suppose do so in fact and perhaps every reason to suppose do not. The fact that we have laboriously to learn perspective drawing is an indication of this, as is the notorious unreliability of eye-witnesses.

That we seldom do describe our experience and then usually with difficulty does not entail that we could not set up and become proficient in the use of a private language. But it would involve a remarkable change in our attitude to the world. Normally we observe in a context of beliefs about where we are and what we are doing that the sophisticated naïveté of phenomenology would exclude. To attend to one's experience involves a radical shift in attitude, a determined effort to resist the solicitations of that submerged constellation of beliefs within which our perceptual discoveries are made.

To this extent, then, I am in sympathy with those who have argued that if the stick half in water looks bent then something really is bent. When I say the stick looks bent, I should discover, if I were to direct my attention to it, that my visual field contained a bent brown line. Whether it follows from this that I am in some way aware of this feature of my visual field is a question that will be answered later. But there is something to be said against this line of argument which is commonly ignored. No doubt when the stick looks bent, something else is bent. But consider these cases. I see a small glassy object in a radio shop

and say 'that looks like a valve'. But in fact it is a wineglass. For this error there is no sensory cue; it is the outcome of my general beliefs about the contents of radio shops. Again, I see what is in fact half a pair of spectacles beside a box which I mistakenly suppose to be obscuring the rest. Even when I know better, it still looks just like a pair to me but it is unlikely that my visual field contains anything corresponding to the second lens.

I have been at pains to emphasise the uncommon and sophisticated nature of the description of experience because of the supposed consequence of the argument from illusion, that in every perceptual situation, even if no object is in fact perceived or if objects are misperceived, still something is perceived, our sense-experience. It would seem *prima facie* that one cannot be said to perceive something unless one is in a position to describe it. But I am not in a position to describe my experience unless I am in the appropriate, sophisticated, phenomenological frame of mind.]

Normally if someone says mistakenly that he sees something we are not inclined to say that he really saw something else. We should say of Macbeth that he thought he saw the dagger, imagined he could see it, was under the impression he could see it, but that he did not actually see it at all. [In cases of illusion, as against hallucination, there will be something that really is perceived, but it will be a perfectly ordinary public object, not a private experience.] If I take a piece of mud on the doormat to be a letter, it will be said that what I actually saw was a piece of mud.

[In general, it is not the case, when I am mistaken about what I claim to perceive, either that I am in a position to describe my experience or that I would be said really to have perceived my experience. There are reasons, nevertheless, which have led philosophers to believe that I am aware of my experience, acquainted with it, in such circumstances.]

It is not only when in the hands of those professionally concerned with it that we attend to and describe our experience. We are sometimes forced to do so by total ignorance of the conditions of observation. Waking up in unfamiliar circumstances we may; if no other assumption seems inviting, suppose that the conventional phenomenological conditions obtain. In exceptional circumstances of this kind, as we come round from an anaesthetic for example, a description of our visual experience is a possible answer to the question 'can you see anything?' But it is worth noticing that in such cases we can also say, with even

better warrant perhaps, 'no, just a lot of yellow streaks' instead of 'yes, a lot of yellow streaks'. Only in a very marginal sense is a description of one's visual experience to be called 'seeing' at all.

In a way, then, we can be said sometimes to 'see' our visual experience : when we are trying to describe it or when we are not in a position to describe anything else. But what of the case of a man lying in the sun on his back with his eyes open and his mind far away ? Does he see the blue expanse with shifting white patches on it that he could describe if he were to turn his attention to his visual field ? And what of the man who is carefully watching a hen to discover where the gap in the hen-run is ? Does he see the green expanse of the downs beyond, that he would in fact find occupying the greater part of his visual field if he were to attend to it ? Compare these cases with a less problematic kind of seeing. Suppose you show me round your garden and afterwards ask me 'did you see the tulip tree ?' If I say 'no', you may say 'you must have done, it's right beside the summer-house I showed you'. If I still deny seeing it, even after another look to refresh my memory, then I cannot have seen it. Yet one might be inclined here to think that I must have seen it all the same. There it was, ten yards away, in broad daylight, right in the middle of my field of vision. But perhaps I was concentrating on the summer-house or thinking of something else altogether. One's visual field is in much the same case as the tulip tree in this example. However far one's attention may have strayed, it seems, nevertheless, that one is inescapably *confronted* by it. So philosophers have said that whenever we think we see anything we really do see the contents of our visual fields. But this is an extremely hypothetical kind of seeing. All we can say is that if I had been in a different frame of mind I should have noticed the tree ; I should have been able to describe the contents of my visual field.

In every perceptual situation, then, we know what appears to be the case, but this is not usually to be in a position to describe our experience. [It may be true that we can be said to have sense-experiences in every perceptual situation (they are, no doubt, the *causes* of our inclinations to believe) but this is quite another matter from being aware of them, noticing them, being in a position to describe them, and nothing less than this can be involved in the claim of the sense-datum theory that it is our experience which we really perceive.]

But can having experiences and being aware of them be clearly distinguished in this way ? For having an experience is a

mental event of the kind, it would be argued, the only direct evidence for whose existence is its presence in consciousness. One might distinguish two senses of 'awareness'. In the wider sense I am aware of any mental event that I am in any way conscious of. In the narrower sense I am only aware of what I notice or attend to, of what I am in a position to describe, of what, in fact, I have some statable knowledge of. Now it might be argued that one was aware of all experience in the wider sense and that this was sufficient reason for saying that all experience was really perceived. I do not think that this distinction can be maintained. It is not that we are really aware of a great many things which we do not notice or attend to but rather that we suppose ourselves to have a great deal of experience for whose existence we have little or no direct evidence. For ordinarily 'be aware of' and 'notice' are largely interchangeable. Both imply claims to knowledge. There are differences of nuance: to become aware of a smell of decay is to have it borne in upon one, to notice a smell of decay is to have discovered it. In implying claims to knowledge both words resemble the perceptual verbs 'see', 'hear', etc. One cannot be aware *of* something without knowing something about it, being aware *that* something is the case.

Now we are, perhaps, usually vaguely aware of the character of our experience, but far too indefinitely for the knowledge involved to support the complicated structure of beliefs that the sense-datum theory would erect on it. The faint and undetailed nature of this underlying awareness of experience is attested to by the fact that when asked to recall our experience we have more or less to reconstruct it from the objects perceived. We attend to experience often enough to know the sort of experiences normally associated with various kinds of object in various conditions. When we transfer our attention from objects to experience an enormously richer awareness of the latter is obtained. We then suppose that we were in fact having experiences of as complex and detailed a kind while attending to the objects, although we were unaware of the complexity and detail. This move is not inference supported by recollection, but a convention. It is assumed that, given unchanged objects, medium, and sense-organs, a change of attention brings about no change in the associated experiences. The idealist's problem 'does attention alter its object?' is thus a matter of convention not of fact. The convention described here lays down that it does not. By this a distinction is introduced between experiences which we have and which we are aware of. It gives a sense to

the expression 'unnoticed experience'. One could equally well, if not better, opt for the other alternative and speak, not of 'unnoticed', but of 'possible' experiences, that is the experiences one would be aware of were one to adopt the phenomenological frame of mind. There is a close analogy with the problem of unsensed sense-data. Should we speak with Russell of 'sensibilia' or with Ayer of 'possible sense-data'? In each case considerations of continuity urge one convention, conceptual economy and epistemological rigour the other. In our problem continuity makes a stronger claim. For while there is a clear distinction between sensed and unsensed sense-data, there would seem to be an unbroken continuum of grades of awareness. At any rate to have an experience of which one is not aware is not so much an event as the possibility of an event, it is to be able, by appropriately directing one's attention, to become aware of an experience. The nature of these possibilities is discovered inductively. I conclude that, whether we decide to say we have experiences of which we are not aware or merely that we could have them, anything we can say about them or their possibility depends on the limited number we are aware of. It is only these, meagre or absent in most perceptual situations, which we can be said to perceive.

III

I have argued that experience cannot be the sole object of acquaintance since it is not the case that in every perceptual situation we are aware of it. If this argument is accepted it can be reinforced—if not replaced—by considering what is *meant* by saying that experience alone is the object of acquaintance. I shall first consider the view that this is so because only of experience can we have certain knowledge.

That statements about objects can never be certain (an elliptical way of saying that we can never know for certain that they are true) is sometimes affirmed on the ground that they are empirical. For it is an essential feature of empirical statements that they can be shown to be false and, it is argued, if a statement can be false there can be reasonable doubt of its truth. But if there can be reasonable doubt of its truth it cannot be certain. This argument has the notorious consequence that only necessary truths can be certain. This is not, as some have argued, merely inconvenient in assimilating one useful distinction to another, it is the outcome of a definite mistake. For it is not correct to say that a statement is certain only if there *can* be no reasonable

doubt of its truth ; a statement is certain, rather, if there is no reasonable doubt of its truth.

This familiar argument, in trying to prove that no empirical statement is certain, tries to prove too much. For, if it were correct, the supposed difference in epistemological status between objects and experiences could not consist in a difference in respect of certainty between the statements describing them. I shall consider two arguments designed to show that, in fact, there is always reasonable doubt about descriptions of objects. Both assert that descriptions of objects have implications which inevitably 'go beyond' or 'lie outside' the current observation.

The first holds that there is no limit to the set of other statements which follow from a given statement about objects. For at any time, however remote from the time to which the original statement refers, evidence will exist and could be obtained for or against it. If at any time there is no evidence, however tenuous, for or against it, it is then untestable and, therefore, without meaning. At any rate the possibility of evidence arising for any statement, however remote its reference, cannot be ruled out. So, it is argued, however much favourable evidence for the truth of a statement may have accumulated, it is always possible that all the evidence to come may point to and, in the end, enforce the opposite conclusion.

If, as I shall argue later, it is also the case that descriptions of experience can be revised, that there can be evidence for and against them distinct from the occurrence of the experience itself, then precisely the same argument can be applied to them and so no difference in epistemological status is established. In effect this argument comes to the same as the previous one ; revision in the face of unfavourable evidence is as much a universal feature of empirical statements as falsifiability.

But, waiving this point for the moment, the argument is fallacious in concluding that statements with 'open consequences' are never certain. For if the statement of unfavourable evidence q is remote, in the way described, from the original statement p , then q alone will not entail the falsity of p but only in conjunction with some generalization or law of nature r . So q will only falsify or disconfirm p to the extent that r is accepted as true and applicable. It is not p and q simply that are incompatible but p , q and r . If q turns out to be true we are not therefore compelled to abandon p . The more remote q is from p , the more tenuous the connexion, the more we shall be inclined to abandon r . This critical point between abandoning p and abandoning r in

face of q may be hard to locate, but for every statement it will exist and for every statement circumstances can be indicated in which its 'logical neighbourhood' is so densely populated with favourable evidence that no remote unfavourable evidence whatever would be taken as refuting it.⁷ So it does not follow from the fact that the set of a statement's consequences is open that there is always reasonable doubt of its truth.

The second argument about implications asserts that statements about objects are always and necessarily predictive, that they always logically imply something which the current observation is not sufficient to establish.⁸ A statement about objects always forms part of a system of beliefs of varying size, at least including assumptions about the normality—or controllable abnormality—of the conditions of observation. But this has no disastrous consequences. In the first place, no infinite regress is generated. The entailed consequences (or assumptions about the conditions of observation) are themselves statements about objects, but *their* entailed consequences (or conditions) will not all be distinct from the original statement. The implications do not fray off endlessly into the unknown, they are, rather, elements in finite, and indeed decently small, systems of mutual support. And in the second place, arising out of this, it is wrong to regard statements about objects as necessarily predictive under all circumstances. For it is perfectly possible to establish all the members of such a set of mutually supporting statements. Knowledge of the conditions of observation constitutes just such a framework which a statement about objects completes, supports and is supported by. I am not here going back on my earlier criticism of the coherence theory. These coherent sets of statements are not self-sufficient. For their members are conventionally correlated with observed situations. Loose talk about semantic or ostensive rules has ignored the indeterminacy of this correlation, the existence of slack in the application of statements about objects which the systems in which they figure take up.

In the normal course of events it is not that the entailed consequences or conditions are yet to be discovered but that they are known already.⁹ This 'systematic' character of our knowledge of objects does indeed distinguish it from our knowledge of experience, consistently with what has gone before since it is the logical correlate of the perceptual as against the phenomenological frame of mind. In the extreme, limiting case (waking up, etc.), where we have no knowledge of the conditions, all descriptions of objects are likely to be less than certain. But

we are not usually in this unfortunate position and single observations can give us certain knowledge about objects.

Even if statements about objects were never certain this would not prove them to be derived from statements about experience, if being less than certain were not identified with being probable and if it were not held that nothing can be probable unless something else is certain.

The crucial error in these interconnected doctrines is the supposition that certainty and probability are exhaustive as well as mutually exclusive. Any assertion made with full confidence may be called certain but only one kind of assertion made with less than full confidence is called probable. 'It appears to be cloudy over there' is perfectly good, if weak, evidence for 'it will probably rain'. Yet the whole point of saying that it appears to be, rather than that it is, cloudy over there is to indicate lack of confidence, uncertainty. That is, a less than certain conclusion can be based on less than certain premises which are not themselves the result of inference. The word 'probably' qualifies assertions which are both tentatively advanced, held to be less than certain, and are the conclusions of inferences. This latter characteristic allows us always to challenge, to ask for the reasons for, a statement that something is probably the case and warrants the view that probability is always relative to evidence. But this evidence may itself be tentative and less than certain. To express just this 'uninferred' hesitancy is, as was shown earlier, the principal office of the words 'look', 'appear' and 'seem'. But can we describe experience in this way? The sole use we have for forms of words where these verbs are reiterated (it seems to look ϕ) is where neither verb is used to describe experience (I am inclined to think that most people would say it was ϕ). But this does not entail that phenomenological uses of these verbs cannot be tentative, that 'this looks to me, here, now, ϕ ' must be certain. To modify these we use adverbial devices like 'roughly', 'more or less', 'sort of' or add the rider 'I think'. We avoid 'appear' and its kin because they suggest assignable reservations, that we realise or suspect something to be amiss with the conditions of observation or, in non-perceptual uses ('he appears to have died about 300 B.C.'), that we realise that better evidence could, in principle, be obtained. But there are no better conditions in which to describe our experiences than those in which they occur, no better evidence than that they occur. The corrigibility of a statement, in other words, does not entail that 'appear' and the rest apply to it; they apply only where assignable reservations are indicated.

Less than certain statements are not all probable ; they are so only if they are the conclusions of inferences, and the premises of these inferences may be less than certain without themselves being inferred. They will be what appears to be the case if I can assign the reservations from which my tentativeness arises or what is, I think, roughly the case, if I cannot.

Finally we must consider a familiar argument against the view that all descriptions of experience are certain. A statement of fact must be expressed by a sentence containing a predicate, a general or descriptive word, and must, therefore, involve the classification of what it refers to, the discrimination of this from other things to which the predicate does not apply. Things, including experiences, do not confront us already sorted out, classified, discriminated. And like any other learnt, regular procedure classification can be carried out wrongly. The use of predicates in classifying and discriminating is essentially a matter of relating what we are describing to the things which are the standard for the application of the predicate, with which it is conventionally correlated, by which it is 'ostensively defined'.

For we can and do revise our descriptions of experience, however convinced we were of their correctness at the time we made them. Such revision could only be excluded by the presumption that recollected experiences, formerly described as ϕ , and now recalled as noticeably different from something else we want to call ϕ , must always be misrecalled. But our recollections have a credibility of their own which does not depend on what is recollected matching something which we now describe with the same predicate we applied to it. Not only can we revise past descriptions of experience, we can also be hesitant about present descriptions. Sometimes we can find no precedent for a perfectly distinct and definite but unique impression ; sometimes, while inclined to give a certain description, there is some peculiarity in the situation which we cannot precisely identify and which makes us hesitate. There is a range of cases between these extremes of inadequate vocabulary and indistinct experience.

Against this view it is argued that the errors corrected by such a revision are merely *verbal*. 'All that one can properly mean . . . by saying that one doubts whether this (sense-datum) is green is that one is doubting whether "green" is the correct word to use.' (Ayer). But what else is one doubting when one doubts whether this *object* is green ? There is a difference, of course, in that one can have another, better, look at the object but not at the sense-datum. But it does not follow from this that all mistakes that do not depend on unfavourable conditions

of observation are not really mistakes at all. What, after all, is a 'merely verbal' error? Properly speaking, only mistaken expressions of belief due to slips of the tongue or pen or laziness and inattention. Linguistic incapacity, the source of mistaken descriptions of experience, is quite another matter. Professor Ayer has recently argued that experience is described 'not by relating it to anything else but by indicating that a certain word applies to it in virtue of a meaning-rule of the language'. The suggestion is that the application of meaning-rules is such a simple matter that it is impossible to perform it wrongly except by a slip. But meaning-rules do not have the bemusing simplicity of their 'semantic' formulation (the word 'red' applies to red things). The class of things to which a predicate applies is indeterminately bounded. Some blue things are more obviously blue than others. Again we are not equally and perfectly accomplished in the application of all predicates. We can manage 'red' and 'round' fairly well, but are less efficient with 'mauve' and 'rhomboidal'. Even if we were trained up to the highest pitch of descriptive efficiency with the predicates we do understand, it is wrong to imagine that that notoriously blunt instrument, our descriptive vocabulary, would provide a precisely appropriate caption for every situation, that it could deal exhaustively with the fecundity of experience. Behind this theory of semantic rules lurks a pair of metaphysical assumptions : that universals, in one-one correlation with predicates, are wide open to some kind of direct apprehension and that there is a decent limit to their variety. The implied analogy with the rules by means of which the truths of mathematics and logic are established is misleading. These rules are precise, definite and can be clearly stated and communicated ; careful tests can be made of whether they have been employed correctly. No such laborious check of the correct employment of 'meaning-rules' is possible with the private, fluid and unstable constituents of our sense-experience.

Lack of clarity about the relation between the mere occurrence of an experience and its description has contributed to the view that we cannot, without lying or slips, misdescribe experience. Experience just happens. But being what it is we cannot help being aware of it. Yet it occurs in every perceptual situation. This confusion of the phenomenologically scrutinised with the more or less hypothetical unnoticed experience is responsible for the view that simply to have an experience is to know it for what it is. Those who have, consistently enough, denied that experience as such is properly speaking either a kind of knowledge

or true or false at all, have avoided the confusion at the cost of abolishing their problem. For from mere events nothing can be logically derived ; only from statements, from what can be known to be true, can other statements be inferred.

[I conclude that statements about objects and about experience are sometimes certain, sometimes not. In this respect there is no sharp distinction between the two.] Whether a description of objects is certain will depend largely on the circumstances in which it is given and what is known about them. Its familiarity and stability will no doubt determine whether a description of experience is certain. We can err about both from linguistic incapacity and the loose correlation of language and the world, about objects on account of unfavourable conditions of observation and about experiences (and occasionally objects) on account of their evanescence. Such difference as there is between the respective sources of error is not sufficient to substantiate a theory of acquaintance or to show one category to be logically prior to the other.

IV

Some philosophers, realising that certainty as a criterion of acquaintance or basic statements is not sufficient to distinguish objects and experience in the way the sense-datum theory requires, have proposed a different definition in terms of inference. On this view we know directly, by acquaintance, what we know without inference ; basic statements are primitive, uninferred ; and, while no descriptions of experience are inferred, all descriptions of objects are. The task of theory of knowledge, it is held, is to make a rational reconstruction of our knowledge of matters of fact in which the uninferred premises from which alone this knowledge can be validly derived are explicitly set out. It is agreed that we are rarely, if ever, conscious of carrying out these inferences. It is thought, nevertheless, that experiential premises must somehow 'underlie' what we believe about objects.

If this account is correct two conditions must be satisfied. Statements about experience must count as reasons or evidence for statements about objects and they must in some, no doubt rather obscure, sense be accepted by those who make statements about objects.] This second, seemingly platitudinous, requirement deserves emphasis. A fact cannot be a man's reason or evidence for an assertion unless, however implicitly, he is aware of

it. Someone's implicit or unconscious awareness of facts about objects can be established by observation of his behaviour. But there is no such criterion available for detecting his awareness of his experience. The view, mistaken as I have argued, that we cannot help being aware of our experience no doubt explains why it has not been thought necessary to provide any criterion for the occurrence of this supposed awareness. If my argument against the view that in every perceptual situation we are aware of our own experience is accepted, it follows that the second condition of the inference theory is unsatisfied and that the theory is mistaken. For our experiences could only be our reasons or evidence for our beliefs about objects if we were to become aware of them through adopting a completely different, phenomenological, frame of mind in our traffic with the external world. Like any other facts, facts about experience must be discovered before they can be appealed to. But even if my argument on this is not accepted, the inference theory is mistaken since the first condition mentioned is not satisfied either.

The best proof that statements about experience were reasons or evidence for statements about objects would be that we did in fact commonly infer from the one to the other. This, however, is admittedly not the case. But, as it stands, this is of little importance. In the first place, the psychological criterion involved is exceedingly vague, seeming to do no more than mark off as cases of inference those in which a thoughtful pause supervenes between observation and announcement. Furthermore, there are many cases, unquestionably of knowledge by inference, where it is not in the least likely that any conscious process of reasoning has taken place. A girl, sitting in the drawing-room, hears the front door slam and says 'Father's home'. I hear a patterning on the roof and say 'it's raining'. I see a small pool on the kitchen floor and say 'the dog has misbehaved'. We only infer consciously in situations that are unfamiliar or complex, in the predicament of the weekend guest or the new boy on the first day of term. The detective, the busybody, the scientist are more or less professionally concerned to make the most of a small stock of data. Conscious, deliberate thinking is both exhausting and infrequent, a last resort to be appealed to only when all habitual capacities have failed. But most of our perceptual knowledge is of familiar states of affairs and acquired in familiar conditions.

That a statement is employed as a premise in a conscious process of reasoning is not the only feature of our use of that statement which shows it to count as a reason or evidence for the

conclusion. More fundamental surely, is that we *give it* as our reason when challenged on the other.

Consider these five cases. I can at once reproduce the course of reasoning that led me to say that it is Mother's hat on top of the garage. This is conscious inference, where the reason given is a premise already consciously affirmed. Secondly, I can, without hesitation, answer 'by the way he sways about' when asked how I can tell someone is drunk, although I recollect no process of inferring. Thirdly, I may take some time over or require assistance in accounting for my claim that Towzer is ill by the glazed look in his eye. Fourthly, I may be unable to give any reason of my own and unwilling to accept any reason offered by another for my assertion that X dislikes Y. Yet commonly in this type of case I may be sure a reason does exist for my belief, may be extremely confident of the truth of my belief and turn out, in the end, to be quite right. Finally, consider standing in broad daylight three feet away from a large and perfectly normal chestnut cart-horse and saying 'that is a horse' or, more adventurously, 'that horse is brown'. This resembles the previous case in that one would be quite unable to give or accept any reason whatever for one's assertion. It differs from it in that one would not be in the very least abashed or apologetic about this. For, in these conditions, the challenge 'how can you tell?' is simply devoid of sense.

Still, if it were made, one might perhaps answer 'well, because it looks like a horse'. If this were intended as a description of one's experience, as interchangeable with 'there is now a shiny brown patch of a characteristic shape in the centre of my visual field', it would not be to answer the question but rather to change the subject, perhaps to offer a causal explanation of one's belief. But this interpretation proposed by the sense-datum theory, a wildly unnatural interpretation of what is, in the circumstances, a wildly unnatural remark, is surely mistaken. The statement would more naturally be intended and understood as a modification of, an infusion of tentativeness into, the original claim, expressing a lack of confidence inspired by the nagging question. As such it is not a reason. To repeat oneself in a more cautious way is not to substantiate but merely to attenuate one's original assertion. 'It looks like a horse' resembles 'it is probably a horse' or 'I think it's a horse' and not 'it has thick legs and no horns' which might be advanced to support the claim that some comparatively distant animal was a horse. For there are, of course, plenty of situations in which reasons do exist for statements about objects.

[A statement cannot be inferred, then, if no reason or evidence for it exists, or, more exactly, if it does not make sense to ask for or give a reason for it.] Whether or not it does make sense to ask for a reason depends on the circumstances in which the statement is made. The sentence, the form of words, 'that is a horse', may be used in an enormous variety of circumstances. In some of these it will make sense to ask 'how can you tell', in others not. The latter may be called the standard conditions of its use. It will be in such circumstances that the use of the sentence will normally be learnt. This accomplished, it will be possible to use it in an increasingly adventurous way in increasingly non-standard conditions. Connexions are established between assertions and their reasons through the discovery of a vast array of factual concomitances. That standard conditions are those in which we learn how to use a sentence helps to explain why the statements they are used to make are basic and uninferred. For in these conditions they are directly correlated with an observable situation, they are not introduced by means of other statements. (This explains 'implicit inference'. I implicitly infer, acknowledge a reason for, a statement if I was introduced to it by means of other statements but can now make it without conscious consideration of them.) For some sentences there are no standard conditions (generalizations or such implicitly general sentences as 'she is naturally shy'). With others the nature of their standard conditions may vary from person to person. A wife will be able to tell at once that her husband is depressed where others have no inkling of the fact. (A difference in capacity that leads us to speak of intuition.) Again prolonged success in a certain nonstandard use of a sentence may lead us to incorporate the conditions of this use into the standard. I say 'it is raining' when I cannot actually see the rain falling but only drops of water bouncing off the wet street. The addition of unwillingness to inability to answer the question 'how can you tell?' shows that these conditions have become standard. Standard conditions are those in which we have a right to feel certain of the truth of an assertion. The suggestion of uncertainty conveyed by the protest 'that's only an inference' would be made more obviously by the equivalent protest 'you are in no position to be sure' (i.e. 'these are not standard conditions'). The lawyer, who asks for a description of what one *actually* saw, devoid of inference and conjecture, is asking for a standard description, that is, a description for which the conditions one was then in were standard.

The notions of acquaintance and of the basic statements which

it warrants have, therefore, a foundation in our ordinary way of thinking and speaking. The failure to locate them in their right place is due in part to the failure to distinguish between sentences and statements. For because of multiplicity of uses there are no 'basic sentences'. What we know for certain and without inference in any situation is what the circumstances we are in are the standard conditions for. This will normally be a statement about objects. But there are circumstances in which, knowing nothing about the conditions or that they are highly abnormal, we can take no description of objects as standard. In such a situation we can do no more than tentatively say what appears to be the case. If we are not prepared to do this we can, by an appropriate shift of attention, describe our experience. This last-ditch feature of statements about experience is another encouragement to the sense-datum theorist.

More important is the fact that standard conditions are not a perfect guarantee of the truth of a statement made in them. For standard conditions do not involve that all of a statement's entailed consequences have been established. The horse in the example may just possibly be a brilliantly contrived deception, a flat painted board. We could make our standard stringent enough to cater for this, by insisting on the establishment of entailed consequences, without abandoning statements about objects as basic. But it would be laborious and inconvenient to do so. The programme of convenience embodied in our actual standards is abetted by the order of nature which is uniform enough to make the risks of standard description negligible. Our standards depend on contingencies but some contingencies are highly reliable and regular. Error, as Descartes pointed out, is a product of the will rather than the understanding and arises almost entirely with nonstandard descriptions.

This minute residual imperfection is the ultimate source of the sense-datum theory. The metaphysical demand behind the theory is for an infallible basis for knowledge. So a new standard is proposed which is thought to be perfect. The justification of the new standard is that the knowledge of conditions required is always available, conditions are always standard for the description of experience. I have argued that we are not, in fact, always in standard conditions for the description of experience but rather that it is always in our power, by an appropriate shift of attention, to produce such conditions. If this is so, the sense-datum theory can be no more than the proposal of a new and exceedingly cumbrous way of thinking and speaking to be adopted from fear of a very minor

risk. But whether it is true or not, whether the sense-datum theory is a proposal or, what it claims to be, an account of what actually occurs, the supposed improvement is illusory. For, in taking steps to set one exaggerated doubt at rest, it provides the opportunity for another to arise. Admittedly descriptions of experience, for which conditions are always standard, do not depend on a knowledge of conditions which may not be forthcoming. But they have weaknesses of their own. The objects we describe are largely stable and persistent; if we are unsure about them we can always look again. But experience is fleeting and momentary; to attend to it again is to make the insecure hypothesis that it has not changed. The systematic, mutually corroborative character of our beliefs about objects is not a weakness but a strength. Similarly the atomic, disconnected character of experiences, which has encouraged the view that they are self-describing entities, is a weakness. I conclude, then, that experiences are not only not in fact the basis of our empirical knowledge but that they would be inferior to the basis we have, since we are just as much open to error about them, though not entirely the same way; and we should have to revise our way of thinking and speaking completely to use them as a basis.

The relation between experiences and objects, then, neither is nor should be logical. On the contrary it is causal, a matter of psychological fact. Our beliefs about objects are based on experience in a way that requires not justification but explanation. Experiences are not *my* reasons for my beliefs about objects—to have an experience is not to know or believe anything which could be a reason in this sense—though they may be *the* reasons for my believing what I do from the point of view of the psychologist. They may, that is, be the causes of my beliefs and explain them. But they could only be my reasons for my beliefs about objects if I already knew something independently about the relations between experiences and objects.

We learn, it is said, to interpret our experiences, to give rein to Hume's principle of the imagination, to apply Kant's schematized category of substance. These forms of words at least point out that perception is an intelligent activity (not an infallible reflex), but they point it out so uncompromisingly that it is over-intellectualised. Interpreting experiences suggests literary scholarship or detective work. But not all intellectual processes are types of reasoning. These phrases refer to the psychological preconditions of recognizing objects for what they are. They point out that we must learn to use the language we do use, that

this is an exercise of skill not an automatism and, further, that the situations in which any one sentence may be correctly uttered are extremely various. But they do not demand and could not evoke any logical justification of our practice of thinking and speaking of a common world of objects. We cannot set out the logical relation of an assertion about objects with the experiences that occasion it, because there is no such relation. This is not to sever language from the world altogether, the sin of the coherence theory. It is simply to say that the relations that obtain within the body of our knowledge do not also connect it with what is outside.

I have considered the three principal methods of establishing the sense-datum theory : the arguments from illusion, certainty and inference. Those who hold statements about experience to be basic have misconstrued all three. Statements about experience are not known in every perceptual situation, for we cannot know what we are not aware of, they are no more certain than statements about objects and they do not differ from all statements about objects in being uninferred. Doctrines about acquaintance and basic statements are the outcome of a search for perfect standard conditions. But no standard conditions are perfect and there is no reason to say that descriptions of experience are or ought to be our standard. Our empirical knowledge already has a basis and as good a one as we can obtain. It is to be found, as we should expect, in those situations in which the use of our language is taught and learnt.

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III.—BETTER AS THE VALUE-FUNDAMENTAL

By M. TIMUR

A. *Objective and subjective values*

WHEN in a concrete situation we make a judgment that the value of a certain thing is based on its own inherent nature and does not depend on its being liked by a person we have an instance of objective value. The value-judgment in this case has universal validity so that whenever a man truly pronounces a thing to be good, it is good for all men and all time. Opposed to this is subjective value which is based on the desire of a person. What the subject likes is good simply because he likes it and only from his own point of view. His judgment is not binding on others and not even on himself in other situations. We make these two kinds of judgment in our daily life often without distinguishing between them, but we can perceive the difference if we carefully examine each situation. There is no doubt that in many instances when we call a thing good we mean no more than that it is liked by a person but we also believe that certain things are good in their own nature without reference to a person.

What appeals to me as a conclusive proof of the existence of objective value is a fact of our daily experience. In many instances value, according to our judgment, does not vary in magnitude in the same way as our desire for the valuable object. Not only does a man judge one thing to be better than another when he likes it more than that other; but also when he likes it less. A man may like the pursuit of pleasure more than the pursuit of knowledge, may actually choose the former and still judge that the course he has rejected is better than the one he has chosen. In the same way we judge one man to be more worthy than another without implying that we love the former more than the latter.

B. *Universally true value-judgments*

We make a large number of value-judgments in our daily life which we regard as universally true and which we believe point to the existence of objective value. As far as I have been able to ascertain they can all be subsumed under four fundamental

judgments which can be expressed in the following form : (1) Pleasure is better than pain. (2) Beauty is better than ugliness. (3) Knowledge is better than ignorance. (4) Creative activity is better than destructive activity. Each of these judgments can be amplified if required in the following way : (a) Pleasure is good. (b) Pain is evil. (c) Pleasure is better than pain. (d) Greater pleasure is better than less pleasure. (e) Less pain is better than greater pain.

Value according to this analysis inheres only in four natural qualities, pleasure, beauty, knowledge and creative activity. The last term requires explanation. I take 'creation' to mean the maintenance by a thing of its own existence or bringing something else into being. Everything that exists in the world is thus a particular centre of creative activity. It either maintains its own existence unaltered or brings into existence new things by combining in causal relations with other things. All physical and chemical action is a form of creativeness and all changes in the world are the result of the creative activity of Nature.

Creative activity in the organic world is on a higher plane. All organisms consciously or unconsciously aim at self-preservation, growth and the preservation of the race by reproduction. Human beings consciously aim at their preservation, the development of their bodies and minds and the multiplication of their race. Individual men help other men in maintaining and developing their life. Unconscious human appetencies and conscious desires are the initial stages in the manifestation of human creative activity. When we say that all creative activity is good and all destructive activity bad, we imply that the existence of everything is good and its destruction bad, the satisfaction of every physical and biological tendency and human desire is good and its non-satisfaction bad.

Although creative activity is always good in itself and destructive activity bad, the former may sometimes bring into existence evil things such as ugliness, pain and destructive agencies and may be on the whole evil ; while the latter may destroy destructive agencies, painful states and ugly things, and may on the whole be good. These evil things may be created by physical Nature, plants, animals or human beings. Such are earthquakes, floods, fires, poisons, microbes of infectious diseases, wars, etc. We should therefore distinguish between creative activity when it aims at pure good and when it tends towards evil. We may call the former moral creativeness and the latter immoral creativeness.

Moral creativeness comprehends all human actions which voluntarily aim at the creation of good. As all good things fall under these four heads, moral creativeness aims at the creation of the greatest amount of pleasure, beauty, knowledge and moral creativeness itself in the largest possible number of individuals. The most important instance of moral creativeness in human life is the establishment of societies which secure peace and freedom for individuals, so that each is able to pursue his happiness, cultivate his intellectual and creative powers and co-operate with others for ends beyond his individual capacity. The most important attitude of the individual will which makes co-operation possible is love and the one which makes it difficult is hatred. Love, benevolence and altruism, therefore, lead to morally creative activities and hatred and selfishness to destructive activities. Love is in fact the very fountain head of moral creativeness.

Moral creativeness of another kind is found in the cultivation of the arts such as painting, music, poetry and sculpture which aim at the creation of beautiful and pleasure giving objects. Their first aim is to produce these objects though an equally important aim may be to benefit human life. The invention of machinery is also an example of creativeness. It places power in the hands of men which may be used to produce much good. Power may also be used for evil purposes and the situation in which it is used may be on the whole evil, but power itself as potential creativeness is good. The qualities of the will such as courage and perseverance are good because they signify the strength of creative activity. They may, however, have evil results and situations in which they are manifested may be on the whole bad.

The idea of creative activity as a kind of intrinsic value unites the concepts of intrinsic and instrumental value. The instrumental value of a thing is its power to bring about results which are good in themselves. Therefore, when a thing is instrumentally good it is also intrinsically good and has two kinds of value. One is its own value as the creator of good results and the other is the value of the good results themselves. Some intrinsically good things which are also instrumentally good possess value of three kinds. For example, pleasure is good in itself as pleasure but when it promotes health and increases vitality, it is also good as the cause of health and vitality. The third value is that of health and vitality.

C. The common idea underlying objective values

If the value-judgments analysed in the foregoing section are not based on the desire of the subject and are universally true, as I believe they are, we may proceed to investigate the nature of the common idea which underlies them. The problem is, what do we mean when we say, pleasure, beauty, knowledge and creative activity are good and pain, ugliness, ignorance and destructive activity are evil. What does the word good or evil mean when predicated of different subjects? The most common explanation when the same word is predicated of different subjects is that they possess a common quality. When, however, we compare things or actions which we call good such as pleasure, knowledge, beauty, creative activity, love, heroic and adventurous activity we fail to find a common natural quality in them. This may lead us to think that the term good refers to a unique ethical quality which cannot be identified with any natural quality. The difficulty cannot be overcome by this method for we feel nothing but confusion when we try to form an idea of the unique ethical quality.

Another explanation of objective value is based on a modification of the subjective theory. It is said that objective value is based on a higher kind of 'will' which is distinct from the ordinary desire. Whenever it manifests itself in different men it has the same objects and so a value-judgment truly made is universally valid. The difficulty which this theory has to face is similar to the one experienced by the theory of 'unique ethical quality'. It is not possible to differentiate the higher 'ethical will' from the ordinary desire. This may lead some thinkers back to the subjectivist position but an examination of objective values shows that there is no justification for taking this view. The reason for the failure to find a satisfactory basis for objective value is that we look in the wrong direction.

The fundamental fact in morals with which every man is familiar is that he can arrange all things of his experience in a scale of excellence. He places some above, some below and some on the same level with others. Our judgments extend over the whole of creation. We can compare animals, plants and physical substances with human lives and with each other. We can also compare the actions of physical things with animal and human actions. We regard some of these as better or higher than others. All these comparisons are made in concrete situations and a judgment pronounced in cool deliberation with a complete knowledge of all the aspects of the things compared is the final

determinant of their relative place in the scale of excellence. The four fundamental propositions under which I have subsumed all concrete judgments are the result of generalisation from these.

The error we commit when investigating the nature of objective value is that we try to go beyond this fundamental fact. We either look for a common ethical quality as we look for a common physical quality in a class of physical objects or we idealize the common desire. If, however, we carefully examine the comparisons of value which we make in our daily life we shall find that we need not go beyond them for an explanation of objective value. We shall find that the most fundamental concept in our judgments of value is that of betterness which may also be called excellence, superiority or moral height. All these words refer to the fact that one person, thing, action or situation is superior to or higher than another. The idea of one thing being superior to another cannot be analysed any further and is one of our ultimate modes of thought. If our thought corresponds to reality it may be called one of the ultimate modes of being. Its opposite is inferiority. When A is superior to B, B must be inferior to A. Superiority and inferiority are two aspects of the same relationship as convex and concave are two aspects of the same curve. They are elementary concepts which are *sui generis* like existence and non-existence; identity and difference; before and after; more and less. They cannot be deduced from each other or from some other simpler elements. They are two aspects of the same fundamental relation which we find in our scales of value.

The relationship of superiority-inferiority pervades every scale of value from the top to the bottom. However, when we proceed from the highest to the lowest term or *vice-versa*, we have to pass through a point which divides the scale into two parts each of which has some characteristics peculiar to itself. In the upper part the terms are superior to one another by a gradual difference. In the lower part they are inferior to one another by a gradual difference. But when we cross the supposed line between the two parts we find that when coming from above downwards there is a sudden fall and when going from below upwards there is a sudden rise. These two parts correspond to what are commonly called good and evil or value and disvalue. In the upper part, even when a term is inferior to another, we are conscious that it occupies a high place in the scale. In the lower part, even when a term is higher than another, we are conscious that it occupies a low place in the scale. We feel while we are in the upper part that we are on a high level, how-

ever low we may be as compared with other things in the scale. When we are in the lower part we feel that we are on a very low level, however high may be the place we occupy as compared with other things in this part.

Concrete examples will make my meaning clearer. Let us examine the scale of pleasure-pain. It is a scale of excellence, for greater pleasure is superior to less pleasure and the least pleasure to pain. The enjoyment of the greatest pleasure is the highest term in the scale. We may move from it downwards to the enjoyment of the least pleasure. Each state of enjoyment is inferior to the one preceding it by a small difference. But when we pass from the enjoyment of the least pleasure to the suffering of the least pain there is a big break in the gradual movement of excellence. Commonsense has, therefore, given the two parts separate names, *i.e.* good and evil, value and disvalue. We may call the upper part in keeping with our terminology a state of elevation and the lower part a state of degradation. By these terms I only mean that the upper part is much more above the lower part than their being parts of the same scale might indicate. The enjoyment of the least pleasure is a high state of being, although it may be inferior to the enjoyment of greater pleasure. The suffering of the least pain is a low state of being although it may be superior to the suffering of greater pain. An examination of the scales of beauty-ugliness, knowledge-ignorance and creation-destruction will give the same results.

There is such a difference between the levels of pleasure, beauty, knowledge and existence on the one hand and pain, ugliness, unconsciousness and non-existence on the other, that commonsense is justified in making a bipartite division of the scale of excellence and calling one good and the other evil. It is, however, unable to specify the ideas conveyed by these two names. If good is a unique quality evil must also be a unique quality, for it is not identical with the absence of good. Nor is it identical with the least good. Like the qualities, pain, ugliness, ignorance and destruction in which it inheres it is a positive character of things. We have already seen that we cannot discern goodness as a unique quality in value-situations. We are equally unable to form an idea of disvalue or evil as a unique quality of objectively disvaluable situations. On the other hand, the idea of one thing being superior to another and one thing occupying a very high place in the scale of excellence and another occupying a very low place in it is quite clear and intelligible. The words elevation and degradation are commonly used in daily life for situations which correspond to good and

evil. They originally mean physical height and depth, but their meaning in the sphere of morals is a unique and *sui generis* height and depth which is distinct from and independent of physical dimensions.

After explaining the fundamental concept of morals as superiority-inferiority, we may define the common terms, good and evil, value and disvalue. The essential nature of goodness or value is the fact that a thing occupies a place in the upper part of the scale of excellence. In other words, it is the fact that it is one of the specially higher or superior things. The essential nature of evil or disvalue is the fact that a thing occupies a place in the lower part of the scale of excellence which is not a continuation of the upper part but is separated from it by a gulf. In other words, it is the fact that it is one of the specially low things.

We thus find that the concept underlying good and evil is the same, i.e. a certain place in the scale of excellence, in other words, the relation of superiority-inferiority. Good is one pole of the scale and evil the other. Like the like poles of a magnet they repel each other and the same thing cannot exist in two places in opposite regions at the same time. Good and evil have, therefore, some characteristics of contradictory terms. The same aspect of the same thing cannot at the same time be good and evil. It will also appear that if a thing is not bad it is in some degree good and *vice versa*. All things must either be in one order or other of the scale of excellence. Take, for example, pleasant and painful experiences. All pleasant experiences belong to the higher order and painful ones to the lower. Every pleasure-pain experience must belong to one of the two orders and none can belong to both.

That there is a common concept which underlies both 'good' and 'evil' is further shown by the fact that propositions in which a wholly good thing is compared with a wholly evil thing in value cannot be properly interpreted without it. Take, for example, the proposition, 'Pleasure is better than pain'. If better is taken to mean 'more good' it would appear that pain is also good in some degree. The fact is, that pain in itself and considered alone is entirely evil. If pleasure is entirely good and pain entirely evil, there is no sense in saying that pleasure is more good than pain, for pain is not good at all. If good and evil are independent notions we can interpret the above proposition only by saying that it is a combination of two propositions : 'Pleasure is good' and 'Pain is evil'. But this ignores the comparison of value between pleasure and pain. 'Better'

in the subjective sense can be interpreted to mean 'I prefer pleasure to pain'. But 'Pleasure is better than pain' indicates an objective relation between pleasure and pain. This can be properly interpreted only if we are able to make a comparison of value between objective good and evil. There is no difficulty in making it, if we regard good and evil as opposite orders in the same scale of excellence and take superiority-inferiority as the fundamental concept underlying both good and evil.

This view of good and evil does not agree with the common view which explicitly or implicitly regards them as qualities of things. But the common view as we have already seen is not based on facts. We cannot discern any such quality in valuable situations. The common view is based on the analogy of value-judgments with judgments about the physical nature of things. 'A is good' is naively taken as equivalent of 'A is red' and then goodness is considered to be a quality like redness. We have already seen that 'A is good' sometimes means 'A is liked by the subject'. Goodness here means the fact that a thing is liked by the subject. In the same way objective value is the fact of a thing being judged as specially superior or high. Common usage prefers short words to long expressions as predicates and therefore we use such words as good, valuable, high, low, superior and inferior whenever the underlying idea completely or incompletely understood is that the subject of the proposition belongs to the higher sphere in the scale of excellence, or is liked by the subject.

It may be said that 'good' is logically prior to 'better', for better means 'more good'. A concept can be said to be logically prior to another when the latter cannot be understood without reference to the former. Such a relation exists between notions when they express more or less of the same substance or quality. Thus stronger, swifter, harder and hotter cannot be understood unless we know their prior terms with which comparison is made in each case. Comparative terms expressing quantity imply the existence of some definite quantity with which the comparison is made. This relation exists between 'good' and 'better' when they refer to subjective or instrumental value, but not when they refer to objective value. Good is prior to better in the subjective and instrumental senses of value but not in the objective sense. Good in the subjective sense is what is simply loved or liked. Better is what is more loved than something else. In the instrumental sense good is what produces a certain required result. A thing which produces the result with greater

efficiency is called better. In both these senses better is equivalent to more good in the quantitative sense. In the objective sense of value, however, better does not refer to greater quantity. It is a unique relation between things which resemble quantitative comparisons in its outward form but not in its essence. There is no good which is prior to better in this sense. Good and evil, on the other hand, can only be properly understood when they are referred to a scale of excellence or the relation of betterness.

Very often we consciously refer to objective excellence but still call a thing good instead of calling it better. The reason for this is that it is more convenient to use a positive term even for a comparative notion. When we use a comparative term we have explicitly to mention both terms of the comparison and determine their relation accurately. When we use a positive term to express a comparative notion we can always omit to mention one term of the comparison. The omitted term is some conventional standard understood by everybody. This standard may vary from society to society or even in the same society according to the occasion. Strictly speaking the least amount of pleasure, beauty, knowledge or creativeness is good when we compare it with pain, ugliness, ignorance or destructiveness. On many occasions, however, large amounts of the first four qualities alone are styled as good and what falls short is called bad. Thus the standard of comfort, knowledge or practical skill called good in a backward country would be called bad in an advanced country. Even in the same society very often when a man calls some person, action or situation as good and another calls it bad they discover in the end that one means better than something A and the other worse than something B. A large number of apparently contradictory moral judgments about the same thing can be reconciled in this way. We may even call pain good when we compare it with greater pain. These instances clearly show that good though positive in form is comparative in meaning. The only difference between good and better is that the idea of relative excellence is implicit in the former and explicit in the latter. It may be noted that good is not the only positive word which has a comparative meaning. The words tall, short, strong, weak, swift, slow, hot, cold and numerous others are of this type. Whenever we call a person strong or a thing hot we mean stronger or hotter than some known standard.

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IV.—CAN HISTORY BE OBJECTIVE ?

BY CHRISTOPHER BLAKE

It is frequently argued that written history can never be objective ; even if the personal bias of the historian can be overcome (which many doubt), it is still inevitable that what is written must be relative to the tastes, customs and prejudices of the creative moment. At worst, no two historians can agree on what really happened ; at best, agreement in one generation fails to survive the next. This argument is sometimes called Historical Scepticism ; it is often derived from and expressed in an Idealist metaphysical account of knowledge as process towards the Absolute, with all judgments the products of a developing consciousness which must itself be regarded as part of the historical sequence. But this is not an inseparable part of the argument, which I want rather to consider as creating a typical philosophical problem about which the following questions may be asked : (a) What kind of objectivity might be expected of the historian, *i.e.* in what way can the choice, in this context, between objective and subjective be given a sense ? ; and (b) Why have so many philosophers and historians found a problem here, *i.e.* why do we take seriously the suggestion that history might never be objective ? I have separated my answers to these questions accordingly, so that in Sections I and II, I consider the sense of the philosophical challenge and thus how it may be met, while in the remaining Sections (III to V) some suggestions about the sources of the philosophical difficulty are made.

I

The Relativist, or Subjectivist, who raises the issue points first of all to certain characteristics of the historian's practice. Wherever the latter may begin, he is forced to select from the total information present to him in records of all kinds, and however he may explain his choice there must be a personal factor involved. Then, although the historian may claim that his final products must, by the conventions of professional probity, contain a factual and therefore indisputable foundation, this is discounted as a naïve suppression of the possibilities of wilfully slanting one's statements of fact, or of accidental equivocation through the vagueness of ordinary words. Thus

Professor H. Butterfield, in the preface to his *Whig Interpretation of History*, may invoke a distinction between "general history" and "historical research", the former alone presupposing ". . . certain methods of historical organisation and inference . . .", only to face a philosophical challenge to show how even the researcher can contrive an objective selection without relying on certain presuppositions purely his own. Or Professor G. N. Clark, in his inaugural lecture on *Historical Scholarship and Historical Thought*, may reassert (p. 20) that ". . . we must not forget that there are such things as facts. . . . All our knowledge of the past has a hard core of facts, however much it may be concealed by the surrounding pulp of disputable interpretation." Yet Levy-Bruhl (quoted by Professor H. B. Acton¹) has asked, rhetorically, "Ne sont 'faits' que ceux qui produisent un certain effet de choc?", i.e. even to employ the term "fact", or "event", is to make a selective, and therefore subjective, move—an estimate of importance. To this Professor Acton himself adds the suggestion that, in the first place, such selection is made by those contemporary with the events themselves, which raises the possibility of a kind of double subjectivity in the material as finally employed. Again Professor Oakeshott (in *Experience and its Modes*, p. 98), using an argument that he applies to activities other than the historian's, asserts roundly: "We know nothing of a course of historical events apart from some system of postulates, . . . What is known is always in terms of what is presupposed"; the point is repeated by him in a review (in the *Philosophical Quarterly*, July, 1952) of Mr. Walsh's recent *Introduction to the Philosophy of History*, when he (Oakeshott) says, of the historian, "he is represented as starting from a 'bare fact', whereas it is safe to say that he never does so, because such a starting-place is impossible—he begins with an interpretation, which he reinterprets".

Another form of attack on the objectivity of history appears in Mr. Walsh's own work, and elsewhere. This admits that historical writing is not entirely a creative, and therefore individual, enterprise. It would concede the picture of the historian's task as painted by such practising historians as Butterfield and Clark; but it assumes nevertheless that the same individual element is a bar to objectivity; and the removal of the bar is seen as a problem of practice rather than of principle. It would admit that historians achieve a degree of impartiality, but deny that history is objective because they do not agree completely. Complementarily, it supposes that a

¹ In "The Philosophy of History", *Proc. Arist. Soc.*, 1939-40.

goal of complete agreement is feasible—even, indeed, that historians themselves believe in such a goal. Thus Mr. Walsh (*op. cit.* p. 98) points out that there are available “apparently inconsistent versions of the same set of events”, and marks the contrast which these make with the historian’s claim to give “if not the whole truth . . . at any rate as much of the truth as can now be come by”. But do historians ever claim this, even figuratively, or in the declamatory sense used in Courts of Law? However, the philosophical attack does not depend on what historians themselves believe, and the point of it is, I think, summed up in this quotation from Dewey’s *Logic* (my own italics): “To be intellectually ‘objective’ is to *discount and eliminate* merely personal factors in the operations by which a conclusion is reached” (p. 44).

Now I think that these arguments can be met, although in a way other than that suggested by their mode of attack. Let it be noted first that among the working canons of historians are standards for determining the accuracy or reliability of sources—standards which are corporately applied. These have been systematised by pedagogues, though usually with mixed success; but, however difficult it may be for the writers of such treatises to bridge the gap between codified theory and recognised good practice, it is indisputable that standards of intellectual honesty are in practice observed. They are observed by common consent, and, moreover, there is common recognition, in spite of some residual disagreement over detail, where they have been transgressed. In short, there is at least a part, actually a very considerable part, of history which is acceptable to the community of professional historians beyond all question by these standards: to Marxists and to Liberals, to Catholics and to Protestants, to nineteenth-century Germans or to twentieth-century Englishmen.

Now the relevance of this to the philosopher’s argument is that, when he denies that history is objective, he cannot be claiming that there is no point in their common subject-matter upon which historians agree. In fact his claim is a peculiar one, for the objectivity which he denies is one that in any case could never be obtained. In either form of attack the philosopher is ultimately relying upon the complaint that written history is not objective because it is relative to the mental climate of an era or to the personal bias of the historian, and yet he pursues the attack by showing this state of affairs to be irretrievable. It is thus impossible that an objective history in this sense could be written, and from this derives the peculiarity, as well as the

force, of the question "Can history be objective?" Standards of appraisal are ruled out of any reply to the question, since being objective is ruled out; for if history cannot be objective as long as it is necessary to select facts, or possible to state them ambiguously or by different locutions, then it never will be. One might say that this brand of philosophical argument shows itself by violating what has recently been christened "the principle of non-vacuous contrast",¹ i.e. the general requirement that no predicate apply either to everything or to nothing in its universe of discourse, since such a rule of use would be tantamount to no rule at all. If a question is asked that pivots on a choice between alternatives, in this case between objective and subjective, then the question can have meaning only if the choice exists. If the choice is *ab initio* ruled out then the question has no sense.

Those arguments already outlined, either that history is not absolutely objective or that it is not objective at all, illustrate the *impasse* very well. If Professor Oakeshott, or Levy-Bruhl, is cited against objectivity in historical writing, then for such writing to be objective we would have to assume one, and only one, objective selection of facts determined by one, and only one, objective set of presuppositions, the whole being recounted in the one objective choice of words. Yet the arguments of extreme historical scepticism, typified in nineteenth-century post-Hegelianism in Germany, have implied the possible occurrence of a situation as ridiculous as this. In Mr. Walsh's consideration, on the other hand, objectivity is certainly made to appear obtainable, simply as a result of discounting those aspects of historical writing which lack the requisite "indifference to persons and places". This, as was said, is given as a practical matter. Yet an objectivity that required such heroic impartiality as this would inevitably be unobtainable in principle, since anything that did achieve it would no longer be history in any recognisable sense. It is, incidentally, part of the philosophical interest of this problem that we are tempted to approach it as a practical one, and then because we cannot see our way to succeeding we are puzzled.

One of the stock answers to Relativism: roughly, that either Relativism is wrong or, if it is correct, then it is itself only a relative verdict; hints at the peculiarity of a philosophical scepticism of this kind. Thus Professor Knox, in his preface to Collingwood's *Idea of History*, says (p. xvii): "If Hegel's

¹ By Mr. C. D. Rollins, in "The Philosophical Denial of Sameness of Meaning", *Analysis*, December, 1950.

philosophy is due to his own psychological make-up or is a function of conditions, economic or other, prevailing in his own time, the same is true of the historian's own methodology and of any possible standard of criticism. In these circumstances questions of truth and falsity cannot arise." Surely this is the trouble with philosophical arguments directed against the objectivity of history, that in the express terms of the argument questions of objectivity or otherwise cannot arise. The historian's canons of regard for the facts, and our everyday ways of assessing objectivity, are both set aside. The *impasse* that results is perhaps best summed up by Max Nordau in *The Interpretation of History* (p. 12), although plainly with a significance not fully intended by the author : "Objective truth is as inaccessible to the writers of history as is Kant's 'Thing-in-Itself' to human knowledge."

II

In this frustrating situation, it is perhaps advisable to consider the ways in which questions about objectivity are decided in ordinary life. To do this, of course, is not to enumerate sundry tests of objectivity ; an attempt to codify rules of use, where by the nature of language no precise rules exist, would be as barren as have been those attempts in the past to codify the historian's accepted ideas about good and bad practice. Rather we should now look for, and study, some typical occurrences of the word.

In the relevant sense where the adjective "objective" is contrasted with "subjective", the following might be representative :

- "Government White Papers are notably objective about such things" ;
- "Can one rely on the objectivity of the 'Times' these days ?" ;
- "Caesar contrived to give a remarkably objective picture of his own campaigns" ;
- "Europe prefers the B.B.C. to 'Voice of America' because it deals more objectively with current affairs" ; and
- "This is the first objective account of Roosevelt's New Deal to appear in print."

Although making estimates of whether something is objective, or how objective it is, may, as in the case of book reviewers, entail some inside acquaintance with that which is under review, yet the bare meaning of the word can be imparted through

certain near-synonyms. Some obvious instances of these would probably come closer than others. It is clear that this meaning is not just respect for the truth, although that is perhaps a necessary condition ; it is unlikely, for instance, that a journalist or historian would be considered to write objectively if he falsified grossly, whether or not he did so inadvertently. But what else may be required when objectivity is demanded cannot, I think, be explained without recourse to a synonym—unless we mark the absence of some characteristic, such as bias or tendentiousness.

Now in the making of this last point two others of importance are implicit : that on our ordinary meaning of "objective" it is indeed appropriate to ask of a particular item of historical writing "Is this objective ?" ; and also that there is a parallel between ascribing objectivity to the historian and ascribing it to the journalist. Taking the latter first : it is, of course, not the case that attaining objectivity involves the historian and the journalist in the exercise of similar skills. But if the critical ascription of objectivity has been learnt with respect to journalism, so that the report of the strike on page 1 is distinguishable from the editorial comment, then a like distinction could presumably be made, say, between a simple statement of rural wage-levels in eighteenth-century France and a study of the causes of the Revolution of 1789. In each case, being objective entails reporting accurately, together with some vaguer notion of neutrality in the idioms used and in the choice and arrangement of what is said—so that we might wish to say of the whole "No reasonable person would argue with that". We would invoke what Karl Pearson called—in another context—"the final touchstone of equal validity for all normally constituted minds". Could there be but one criterion of objectivity, this might be it. Nor, then, could it be objected that concepts like "reasonable" or "normally constituted" begged the question. For such a complaint seems to hold out hopes that the meaning need only be once intuited for it to be pinned down for good. But written or spoken language does not admit such hopes. All that is, indeed can be, said is that to call something objective is to imply that other people—reasonable people, that is—would accept it. To feel that this merely pushes the investigation one place back is to misconstrue, in a potentially calamitous way, the function of an investigation of this kind.

Let us return now to the earlier point, that we can ask of a particular work of history whether it is objective. Does the

title-question of this paper now have an answer? Certainly many people—teachers, reviewers and historians themselves—do recognise that some historical work is objective. Thus there is a temptation to say that, provided that it is not asked whether history can be objective in a philosophical sense that *a priori* excludes the possibility, we can give an answer of sorts. The question could be interpreted as asking for guidance, *i.e.* whether it is appropriate to couple this adjective with that noun (*cf.* Can a dog be intelligent?), in which case the answer, according to the above analysis, would be affirmative. Or we might answer: "Well, if you are asking whether *all* history is objective, the reply is No, but if you mean *some* history, obviously Yes." That is, we would try to take the question at its face-value. But it is extremely questionable whether there could be such a face-value, for even in the most ordinary, non-philosophical, contexts there is something awkward about asking "Can history be objective?" For instance, it would scarcely be appropriate to ask, in any imaginable context, "Can journalism be objective?" Individual works can be objective or not, but there is a feeling of strain in trying to assess the class as a whole. To ask "Can history be objective?", in short, is rather like asking "Can novels be well written?", or "Can anything be known?"

Yet to show this is not to make utter nonsense of the philosophical question. There are undeniable difficulties in taking a reflective view of history, which are emphasised in the arguments that philosophers use to deny its objectivity; these difficulties constitute the subject-matter of the latter parts of this paper, where the sources of the problem are examined. Furthermore, there are difficulties in the ordinary concept of objectivity which, although not directly apparent in philosophical discussion of history, still deserve notice. One of these is the temptation to raise a philosophical question which is merely a more general form of that posed about history. Thus when the objectivity of something or other is in question, it is easy for one side to say: "But can we ever be *really* objective? Surely it is impossible to suppress one's subjective inclinations altogether?" By this road, in fact, might one be seduced into asking "Can journalism be objective?" This general temptation may be at least partly explained later, as may the following usage which seems to be at odds with that analysed. When referring non-scientifically to science, the latter is often called "objective"; it is clear that a comparison with the subjective is being made, *i.e.* there is no vacuous contrast, yet it seems

hard to imagine how any piece of scientific discourse could ever be regarded as subjective in any sense.

To sum up this paper so far : I hope to have shown that, by asking whether history can be objective, philosophers, and some historians, appear to be raising a pointless question, since their arguments against the possibility seem to constitute a *petitio principii*, and since we can give to the question no alternative recognisable sense. I want to show now that the question, although misleadingly expressed, is far from pointless, since it brings out some important philosophical temptations that arise when we regard history in a speculative way.

III

When we ask ourselves what it is to have historical knowledge the most obvious, and probably the most influential, source of confusion is an analogy with natural science ; and it is plain from the writings of those who criticise the objectivity of history that in many cases a desire to assimilate history to the sciences is one source of their complaint. According to a very general code of epistemological respectability the sciences are beyond reproach ; this seems to be so particularly when questions of objectivity are raised, for scientific statements are only too easily pointed out as the very paradigm of impartiality and of indifference to time and place. But history is not a science, and therefore as a paradigm of objectivity for the philosophy of history science just will not do. (The question whether history is a science, and—almost as important—the possible misuse of answers to that question, are examined in Mr. P. Gardiner's recent *The Nature of Historical Explanation*, to which I am in debt.)

Traditionally history was compared with the natural sciences either by subject-matter, wherein some said that they were alike in studying reality and some that they differed in being concerned respectively with men and with inanimate nature, or under the ambiguous notion of method. Occasionally these *differentiae* were thoroughly confused, as in Comte's account of the Social Physics which was to supplant history by right of its “*méthode objective*”. However, if we are to make a comparison that is both unambiguous and philosophically valuable we must abandon these traditional alternatives, and concentrate on a study of the languages which history and the sciences respectively employ. A study of the logic of the sciences—for example, of Physics, which is considered the most mature, and

hence archetypal, science—shows that their ideal structure is that of a formal deductive system. Such an artificial language is built around a logico-mathematical framework, and is consequently possessed of the important characteristics of : (a) conceptual precision ; and (b) explicit rules to govern what can and cannot be said in the language and what does and does not contradict in it. I do not want to enter here into the question of the interrelations between these, nor of the reasons why they are regarded as ideally appropriate to what purposes the sciences are designed to fulfil. However, I emphasise that, although what we call the English language is nowadays an amalgam of scientific and unscientific words, the large body of human discourse is and must be utterly lacking in those specialised and restrictive attributes. It possesses a logical structure infinitely more complex than that given in a formal system, and this complexity shows itself in so many directions that the concept of a "logic" of natural, or free, discourse is apt to mislead those who have the structure of formal systems in mind. Now I want to assert categorically that, while there remains an obsolescent tradition whereby history is called a science (*cf.* the German word "Wissenschaft"), the language of the historian is, and again must be, of a natural, and therefore by comparison indeterminate, sort. The ideal scientific statement appears in a closed deductive system, while it is essential to historical writing that it enjoy the fluidity and adaptability of ordinary language—"ordinary" not in the sense of "colloquial" or "plebian", but denoting a manner of stating and arguing that is common both to popular and to professional forms of speech.

If in this way, then, history is not a science, here is at least *prima facie* ground for expecting different standards of objectivity. But to do so is not to suppose a greater probity or self-control on the part of the scientist, nor is it to say that the historian's subject-matter, lying closer to the heart, is more likely to tempt personal feelings to intrude. The fact is simply that the scientist has less scope for variation, and therefore for personal embroidery, because *qua* scientist there is less that he can say. Whether he says *that* so-and-so happened, or *why* it happened, the scientist uses a terminology that is given precisely fixed and officially standardised meaning, and that is at any one moment severely limited in extent—limited, that is, by comparison with the natural language. Thus, when it is said that science is objective attention is being drawn to this fact : that scientists, in cases where they disagree, have at hand clear-cut

methods for settling their disputes. Now the same is just not true of historical accounts. The richness of natural language allows, and even encourages, disputes of a kind impossible within a systematised context—not the black-or-white disputes with which the latter is designed to cope, but the possibility of saying the same thing in several ways, or of finding a number of competing explanations, and the impossibility of deciding such a dispute (perhaps between historians) on demonstrably impersonal grounds. And if science is popularly considered objective on account of its freedom from these irresolvable conflicts, then history will necessarily appear subjective by comparison ; at the same time the utter lack of analogy, on this sort of basis, between history and science makes such a comparison irrelevant. Earlier I noted an apparent inconsistency in ordinary usage when science was called objective ; it now appears that there are two distinct usages, and that what we mean when we say that science *is* objective and when we say that history *can be* are two very different things.

Of course this ignores, probably with justification, a peculiarly philosophical sense in which science is sometimes called objective. The etymology of that word seems to associate it with the traditional statements of epistemological dualism, wherein to be objective is to be part of the world of objects, the out-there. This is the context of debate over the location of Primary and Secondary Qualities, and it has persisted in a great deal of contemporary discussion of science. There is a strong Lockean flavour about the way in which it is often still thought to be appropriate to elucidate the distinction between cases like these :

“I say, it’s quite warm in here”, and

“At point (x, y, z, t) the temperature is 300° A.”

The latter is objective : which can mean, perhaps, nothing less innocuous than that it is acceptable to everyone who understands the terminology, while the former need not be. But it can also suggest to some that it represents something spatially independent of the speakers, and hence outside the physical range of their personal feelings.

Nevertheless, what has been said should suffice to refute any explicit comparison, in terms of their relative objectivity, of history with science ; unfortunately, however, a great deal of philosophical discussion has apparently relied on the comparison without doing so explicitly. This, I think, is true of Dewey’s *Logic*. Here a phrase is used that seems, in the context, importantly ambiguous : “controlled inquiry”. This can mean an

inquiry that is "logically grounded" so that "individual peculiarities are deliberately precluded from taking effect", which would be an adequate *résumé* of the scientific endeavour for our present purposes. But it is easy to slide, as I think Dewey does, into a vaguer meaning altogether, of any intellectual discipline with a recognisable individuality. Thus the door is opened to admit history as well. A closely analogous ambiguity is perpetrated when Dewey resorts to the terminology of logical theory. In reacting against the suggestion that such controlled inquiries could ever yield more than one right answer, Dewey says : "If the same evidence leads different persons to different conclusions, then either the evidence is only speciously the same, or one conclusion (or both) is wrong" (p. 44). Now if "evidence" is to be understood as "deductive premise" this is trivially true ; it is also true, although in a quite dissimilar fashion, of the inductive aspect of science ; but as a point about historiography it would be totally incorrect. In offering these quotations from Dewey I believe that they illustrate a common fault, and a common path whereby loose thinking can lead to the analogy between history and the sciences which, I suggest, we want to avoid.

Again stemming from this misconceived analogy with science, there is another dimension to the sceptic's arguments. If personal emphasis and variety of expression in the history written at any one time argue against historical objectivity, then so too must the fact that history is rewritten for every generation. Although the sciences are likewise being continuously rewritten, the scientist can offer an apparently satisfactory, because impersonal, cause for these changes. Unlike the historian when he rewrites, the scientist does not seem to be in the grip of hidden forces ; we do not rewrite Newton's *Principia* to satisfy our social or political interests. On the other hand, we do find his formula for gravitational acceleration inadequate for all our occasions—but we give an apparently objective reason for saying so. But this, surely, is once more a symptom of the effectiveness of those self-imposed rules, for accepting or rejecting explanations, which the scientist alone is able to apply because of the logical nature of his discourse, and which makes science alone "objective" in the sense appealed to. If the fact which is expressed so grandiosely by saying that historical judgments are themselves historically conditioned is offered as a philosophical complaint, then the comparison with science cannot be far away. As we are disturbed by irremovable contemporary disagreements in what historians say, so are we

disturbed by their seemingly arbitrary adjustments to yesterday's received interpretations. In either case, it is at least plausible to suppose that we hanker after the clear-cut alternatives that confront scientists in their work.

So it is important to remember that we can admit that standards of historical criticism, and therefore what passes these standards, are in constant flux, without conceding this as a ground for questioning whether history can ever be objective. For to make that concession is to make a concealed shift of meaning, whereby to be objective now means, as it does when applied ordinarily to science, to admit no undecidable disputes. This, as I pointed out, is not what we mean when we ask if this or that piece of historical writing is objective. Indeed, Relativism, once its misappropriation of scientific objectivity is shown up, may be claimed to have indicated obliquely the fundamental truth that history is not a science after all.

Which conclusion has, I think, perhaps not been better expressed than in this remark (quoted by James B. Conant, in *Science and Common Sense*) : "History is not a deductive science and there are no rules for detecting fact. There are rules for detecting fiction, but that is a different thing altogether." Yet I suggest that there is a reluctance to accept this conclusion, or rather what it seems to entail : that there is no ultimate, or timeless, sense in which it can be asked, of any historical account, "Is this what *really* happened ?" Any finality in answering all historical questions seems impossible ; and the mental dissatisfaction, the feeling of puzzlement, which this conclusion brings appear to be not wholly explicable as the after-effects of realising that we cannot talk of history as if it were a science. Philosophical doubts about the objectivity of history, in other words, carry more than the force of a contrast with scientific objectivity. Reflection upon what it is to know what actually happened is influenced also by a persistent idea concerning what it is to know the facts—*i.e.* the fundamentally erroneous idea that something which is known corresponds to something else which it is known about.

Since this topic has been discussed considerably elsewhere, and since generalised remarks about the function of the fact-stating type of discourse seldom enlighten, I shall be as brief and specific as I can. Thinking about such notions as factual truth seems to be dogged by a certain picture of the things which are said truly somehow mirroring that (the physical aspect of the world) to which they refer. Since this picture can never be made explicit it is merely hinted at by such parallels as mapping,

modelling, reflecting—any physical process with the basic characteristic of reproducing a structural form. Somehow we want to think of language as attempting to do this for the world ; saying, as it were, is assimilated to naming ; the true sentence is the one appropriate arrangement of words, and every fresh piece of knowledge is another building block in the verbal facsimile of the world. Today, Correspondence theories of truth and Relational theories of meaning, both of which assume the appropriateness of the picture, are increasingly discredited (which, of course, is not to say that other theories are being promoted to fill the vacancies) ; yet certain integral parts of using language—parlances such as “corresponding to the facts”, the look-and-see kind of verification, or the learning process known as ostensive definition—do give continued life and vigour to the idea that we can match up everything we say with the appropriate segment of Reality as one reads, or checks the accuracy of, a map. Philosophical argument which appeals to the picture, no matter how well disguised, is convincing, for this is one of the hardest of philosophical harnesses to discard.

The supposition that this picture influences thinking about historical objectivity would explain much of our puzzlement. If history is viewed as trying to recreate reality on paper then on a strict Correspondence view it must either succeed or fail. There is no place, in a search for knowledge about the past, for a plurality of contending answers. We do not wish to have to contemplate, let alone accept, a number of right answers, since to do so is to subvert the whole picture we have, perhaps unwittingly, adopted of how right answers are arrived at and how they are to be judged. Professor Mandelbaum, in his forthright attack upon Historical Relativism (*The Problem of Historical Knowledge*), illustrates the point, since he avowedly rests his case on a Correspondence theory of truth. He recognises that scepticism about the historian's ability to yield any objective knowledge of the past is in direct conflict with this theory ; thus he brings out into the open the very conflict which makes those of us who do not explicitly embrace a Correspondence theory nevertheless suffer discomfort at the demonstration—which we cannot see how to refute—that the historian's work is conditioned by personality, politics, class or nationality, or the mental climate of his times.

According to Professor Mandelbaum, “ . . . it is the property of language to refer to non-linguistic entities ”, and among these entities are “ concrete facts ” which have a definite order in themselves. Thus for the historian surveying his material

there is not the freedom of movement that the Relativist hypothesis requires : "the fact itself leads on to further facts without any intermediation or selection based upon the historian's valuational attitudes, class interests and the like." However, this ultimately leads Professor Mandelbaum to his *reductio ad absurdum*. Since "every recognised historical account is a tissue of facts", all disagreements between historians must be factual disagreements ; certainly this follows from such a rigid Realism, but it shows simply that such a Realism leads to a position opposite to but equally untenable with a blanket denial of objectivity. For now there is in acceptable history nothing other than the objective. And the reason for this is that to maintain a Correspondence theory of truth is not to offer an answer (except by way of a rhetorical exercise) to what the Relativists claim. To the philosopher who argues that the historian's search for material is made in accordance with some impermanent interest, of himself or of his period, we should return not a denial of his point but a denial of its alleged epistemological importance. We ought to say not that this is not (in a manner of speaking) what happens in writing history, but that it has nothing whatever to do with the way in which historians credit each other with objectivity and a respect for the facts.

Nor does this exhaust the troubles of the Correspondence view. In this view it becomes significant that the subject-matter of the historian lies in the past. Mr. Gardiner, with his eye especially upon Idealist philosophers of history, has warned against any "bogus mysticism" about the past ; but we can reject the Idealist's juggling with our notions of past and present and still feel the force of the ancient philosophical doubt that asks : How can we *know* that things were as we think they were ? (Indeed, how can we know that there was anything at all ?) In the Correspondence view objectivity is seen as a one-to-one correlation between the account and the actual event ; but what if the actual event is no longer with us ? The kind of picture-making of real events that is history must, then, be a representation of a representation. Any prospect of determining the accuracy of the second representation, of making quite sure that the historian has not coloured to his personal taste at the expense of objectivity, seems to be gone. In the Correspondence view, the only kind of verification is by direct confrontation, and with history this is out of the question.

IV

We may now consider a fundamental pitfall in the critical philosophy of history, wherein two influences already discussed—the comparison with science, and the Correspondence picture of language—are both to some extent involved. In the first section, I quoted the following from Professor Oakeshott: "He (the historian) is represented as starting from a 'bare fact', whereas . . . he begins with an interpretation . . ." Now it would be justifiable, perhaps, to reject this as the merest play upon words, as the abuse of a forensic device. But to do so would ignore the importance of what is indirectly pointed out: that a distinction between that which is expected to be objective in history and that which could not be is at least an extremely difficult one to draw. Professor Butterfield separates "general history" and "research", which seems to parallel the almost-traditional terminology (*cf.* Langlois and Seignobos, *Introduction to the Study of History*) of "synthetic" and "analytic" history; while Professor Clark relies on the form of distinction which Professor Oakeshott rejects when used by Mr. Walsh. But all of these seem to presuppose the distinction rather than illuminate it; this would be the case, I think, with any attempt to make the distinction, since one would be trying to make a general rule out of a fluid practice realised only in particular instances. Historians and their critics recognise that each individual historical work need not restrict itself to a "scissors-and-paste" technique in order to be objective, for the elusive element of synthesis is essential if history is not always to read like a laundry-list. Yet any attempt to make a general bifurcation of history into its objective and subjective elements is apparently unhelpful, and often runs the risk of allowing philosophical play upon the meanings of words to lead us to the absurd conclusion that, after all, objective history does not exist.

Now, of the kind of distinction that is attempted in these cases two questions may be asked: (a) Why do we think that we can make it?; and (b) Why do we feel that we must make it?

In answering the former, the tendency to assimilate historical narrative to scientific explanations will make plausible a great deal. Although for most purposes it is misleading to consider as in separate compartments scientific theories and the data which they explain or predict, the tendency to do so has at least a partial justification in the logic of science. This logic being what it is—and in a sense that is not true of ordinary discourse

it is what we have made it—we can distinguish *explicandum* from *explicans* precisely and unambiguously, so that it is analytically possible to order scientific statements as data, lower-level theories (generalisations), transcendental hypotheses, and so forth. Now some such distinction seems at first sight possible in ordinary speech. It is a fact that the chicken crossed the road, and an explanation that it wanted to reach the other side. But we cannot hope to make this distinction hard and fast. Taking the case of a fact-interpretation distinction : it is clearly possible to find borderline instances which could be decided only by the apparently arbitrary ways in which all questions arising from indeterminate usage are decided in ordinary speech. An example offered by Professor Field¹ shows this : clearly it is a fact that, to the best of our knowledge, Plato visited Syracuse for the first time when he was about forty : but is it fact or interpretation to say that Plato was a pagan because he believed in many gods ?

Alfred Sidgwick, in his *Process of Argument*, noted this tendency to hesitate over such a choice. Of the fact-interpretation pair of alternatives he says (p. 14) : "Whatever value this division has, it does not enable us to set aside a *distinct* class of 'assertions of fact' as above dispute" ; although we may discern, in practice, members of this class, ". . . to admit the existence of such cases, or even their frequent occurrence, is a very different thing from specifying exactly what they are". He cites certain borderline cases, such as : Did Byron ill-treat his wife ? This, like Professor Field's example, is on the borderline because certain usages that are involved are vague (as, of course, is the notion of a "fact"). Sidgwick proposes as a further reason for rejecting the idea of a definable class of facts the possibility of perceptual error ; but this, quite apart from leaning on a fallacious philosophical tradition about what it is to make mistakes in seeing, seems to confuse the separate and distinct difficulties in affirming that a fact is not an interpretation and verifying that it is factually true. The logic of ordinary language, it seems, will account sufficiently for our failure to reproduce for it a distinction which the logic of science admits.

Even now, however, the root of the difficulty over facts and interpretations in history may not have been unearthed. Instead of the fault being simply one of trying to standardise a fluid distinction between facts and interpretations, in order

¹ In "Some Problems of the Philosophy of History", *Proceedings of the British Academy*, 1938.

thereby to make a distinction between objective and subjective elements, it may be that it is one of supposing that the *latter* distinction could, or should, be made at all. This, I think, is precisely the matter. The radical fallacy is to suppose that we can talk in the same breath of that which is expected to be objective in history and that which could not be. For this pair of opposites is an illicit pair, conceding and concealing the ambiguity between the unsophisticated and the philosophical uses of "objective". Hence the whole terminology of different but comparable parts, or elements, of history—the objective and the subjective—stems from the original statement of the philosopher's case against objectivity. Even where we are sensitive to the oddity of the question "Can history be objective?", we are tempted to meet this kind of scepticism head-on. Thus the question is taken at its (non-existent) face-value in an attempt to show that there is a part of history that is objective, and hence safe from this attack. To the challenge "What part?" are produced the indubitable, and indubitably present, facts. But this alleged defence, of exposing the factual strands woven into the thread of history, is not merely to mistake the philosophical attack for a straightforward piece of criticism but also to slip into paying the philosopher back in his own debased coin. For it is in the way in which doubt is originally cast upon historical objectivity that lies the origin of the framework of argument which is now being employed. There, objectivity was questioned by showing either that no part, or only some part, of written history could pass the philosopher's test; it is one or other of these forms of attack, based necessarily upon a partition of what is written, that imposes upon later discussion the need to distinguish fact from interpretation. Moreover, this seems to have been done from some prior assumption about how objectivity is assessed.

When we judge something to be objective we are, somehow, assessing it in one piece, as a whole. If we are challenged to explain our judgment we would not point to certain distinguishing marks—an undeniable fact here, a well-documented piece of evidence there. Rather, we would refer to the general tone, or (as I suggested earlier) to the absence of certain emphases. Thus to talk philosophically about objectivity, as if this were an attribute carried by the "objective parts" of narrative, is to miss an important insight as well as to misdirect discussion in the manner indicated. It is, possibly, to make a concealed identification of the logic of "objective" with that of some other, spuriously analogous, concepts. It may be to assume

that to be objective is no more than to show the desired proportion of "truths". Certainly it constitutes an error, as second thoughts will show, and perhaps the most critical one that a study of the roots of the puzzle over objectivity will reveal.

V

Finally one might observe, in the everyday uses of "objective", certain still unnoticed sources of difficulty. For instance there is an undeniable tendency to employ the word as an encomium; any man's newspaper is usually more objective—to him—than is his neighbour's; the word is used by editorial writers as well as about them, and about reviewers as well as by them. Also the noticeable indeterminacy of the usage has already been remarked, *i.e.* we cannot say with any precision what an objective account of anything would be like. For both of these reasons there can be in practice many cases where the objectivity of a piece of history is interminably disputed, and the suspicion arises that perhaps we can never hope to settle in favour of any piece of historical writing any such dispute. But to accept this is to return to the irremovable, and therefore self-nullifying, scepticism which is the philosophical temptation, and for which the cure is to remember that, before we started to wonder, we did know how to use the word.

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V.—USES OF SIMILARITY OF STRUCTURE IN CONTEMPORARY PHILOSOPHY

BY HIRAM J. McLENDON

I AM concerned here both to describe one of the main technical concepts employed by Russell throughout most of his philosophy, namely, his concept of similarity of structure, and to judge its alleged usefulness for solving a number of important problems in philosophy.

Russell's system, however, is but one of many which are systematically permeated with the concept of similarity of structure. Both the concept of isomorphism defined and used by Weyl and by Cohen and Nagel and Carnap's much trusted concept of intentional isomorphism appear to be closely similar to Russell's concept of similarity of structure. In Eddington, Wittgenstein, and Schlick twin concepts are also employed confidently. We may indeed say that systematic use of the concept of similarity of structure in one guise or another for dealing with problems of epistemology and metaphysics especially characterizes philosophy during the first half of our century.

I find it a convenient expository device to develop my critical evaluation of this concept by reference to Russell's especially conspicuous formulation and use of it throughout his writings. However, I would like it remembered that my larger concern here is to judge the philosophical importance of this piece of technical apparatus itself, not merely as it appears in Russell's system making but also as it is used in much of the system building characteristic of recent philosophy of analysis. Russell's uses of it will be taken simply as a paradigm of this recent innovation in technical philosophy.

If Russell's *applications* of his concept of similarity of structure are to be responsibly judged, his *formulation* of it must first be carefully expounded.

I

To define "similarity of structure" one obviously needs first to define "structure".

"Structure" in Russell's writings is used in at least four distinguishable senses.

In the weakest sense, to say of something that it has a structure means simply that it has a complexity. For example, wholes, classes, and relations are found upon analysis to possess a plurality of parts, members, or terms. In this sense, any entity with a plurality of components may be said to have a structure.

In another sense, to speak of the structure of something is to refer to the manner of arrangement by which its distinguishable constituents are related to one another. In this sense also any entity whatsoever having a plurality of parts will also have a structure. Hence, in this second sense, a description of the pattern of relations holding among the terms or members or parts of the complexes in question is a description of their structures (HK, p. 250: "To exhibit the structure of an object is to mention its parts and the ways in which they are interrelated." Cf. especially AMR, p. 249.)¹

This second use of "structure" to refer to the pattern of relations holding among constituents of a complex, whatsoever the characteristics of the complex and its relations be, divides itself into two important species, which constitute a third and a fourth sense of "structure".

In the third use of "structure", the relation which holds among the terms in the complex and gives it its structure is a nonordering relation. This type covers a very wide variety of cases. For example, the spatial relations which hold among the blocks carelessly scattered over the floor by a child at play and those holding among the automobiles crowded haphazardly

¹ I shall refer to the following writings of Russell by the abbreviations indicated (usually the capital letters of the principal words in the title):

The Problems of Philosophy (New York : Henry Holt & Co., 1912)—PP ;
Our Knowledge of the External World (London : George Allen & Unwin Ltd., 1914)—KEW ;

Mysticism and Logic (New York : Longmans, Green and Co., 1918)—ML ;
Introduction to Mathematical Philosophy (New York : The Macmillan Co., 1919)—IMP ;

Philosophy (New York : W. W. Norton & Company, Inc., 1927)—PHI ;
The Analysis of Matter (New York : Harcourt, Brace & Company, Inc., 1927)—AMR ;

The Scientific Outlook (London : George Allen & Unwin Ltd., 1931)—SO ;

An Inquiry into Meaning and Truth (New York : W. W. Norton & Co., 1940)—IMT ;

"Reply to Criticisms" in *The Philosophy of Bertrand Russell* (ed. P. A. Schilpp) (Evanston, Illinois : The Library of Living Philosophers, Inc., 1946)—RC ;

A History of Western Philosophy (New York : Simon & Schuster, 1945)—HWP ;

Human Knowledge (New York : Simon & Schuster, 1948)—HK .

on a parking lot are nonordering relations in the strict sense of "ordering relations" to be defined presently. In this third sense, the baseball players distributed in position on the field also have a structure. In the same sense the class of points, lines, dots, and colours on a map have a structure. Russell often uses "structure" where it can be interpreted only in this sense (HK, pp. 253-254, 460-475, 491-492; RC, p. 716).

In the fourth sense, to say of a class of terms that it has a structure means that the members of the class are related by an ordering relation (HK, pp. 254-256), that is, one which has the logical properties of transitivity, asymmetry, and connexity. (The doctrine of relations employed in Russell's definition of structure and taken for granted in this exposition of it is developed in a nontechnical form in IMP, especially in chapters i, iv, and vi, and the definition of ordering relations is developed in chapter iv.)

In this fourth sense, the class of points on a straight line taken from left to right are structured. Also, a sentence viewed as a physical occurrence of printed symbols on a single straight line taken from left to right has a structure in the strictest sense (HK, pp. 250-251). In both these cases, the *spatial relation* of left-to-right on a straight line, because it is transitive, asymmetrical, and connected, is an ordering relation and the classes of terms thus related are ordered classes in the strict sense of "ordered class" defined by Russell.

In the strictest sense, a spoken sentence viewed as a sequence of sounds also has a structure, since the *temporal relation* of preceding, like that of succeeding, is an ordering relation. In the same sense, the sequence of experiences of any given individual is a strictly structured class of occurrences ordered by the temporal relations of preceding and succeeding (HK, pp. 250-251).

Likewise, the collection of natural numbers, when arranged in the order of their magnitude, is an example of a strictly structured class, since the relations of magnitude of greater-than and less-than are ordering relations in the strict sense of "ordering relation". (See IMP, pp. 30-31, where Russell explicitly defines "an ordering relation" so that the expression refers to all and only those relations having the logical properties of transitivity, asymmetry, and connexity.)

Because all temporal relations except simultaneity, all relations of magnitude except equality, and some spatial relations are ordering, they give structure to their fields in the fourth and strictest sense of "structure".

It is very important to keep clear these four different senses of "structure", the sense in which "structure" means "having

a complexity", the one in which it means "having a specifiable pattern of relations", the one in which it means "being arranged by a nonordering relation", and the one in which it means "being arranged by an ordering relation".

From these explications and examples we may generalize concerning the interrelations of these four senses of "structure" as follows: Any entity whatsoever which has a plurality of parts will have a structure both in the first and second senses and in either the third or fourth sense but not both.

One very appropriate criticism both of Russell's discussion of structure and of his uses of it is that he does not bother to sort out these distinct senses. As a result, his readers, discovering that Russell does not use "structure" in a single sense, are left to puzzle out the different senses and to decide in given contexts which sense is intended.

The concept of structure formulated above in its variety of senses, needless to say, is an extremely abstract concept. It is abstract in this precise sense, that the structure of a complex is determined entirely by the *pattern of relations* of its terms, not by the nature of its constituent terms themselves. It is also abstract in the further equally precise and derivative sense that the structure of a complex not only may vary independently of the terms structured but also is quite *determinable and describable without any reference to the character of the terms related*. This is equally the case whether the structure be founded by a nonordering relation or by an ordering relation. (For Russell's emphasis upon the extreme abstractness of the concept of structure, see IMP, pp. 60-62; SO, 83-87; HK, p. 256.)

Russell's concept of similarity of structure may now be defined.

Although any single complex may be said to have a structure in one or more senses, similarity of structure is a relation that can hold only between two or more complexes, each of which has a structure. Similarity of structure, therefore, is an even more complex and abstract concept than that of structure and discussion of it correspondingly thornier.

Since there are as many senses of "similarity of structure" as of "structure", each sense requires explication.

"Similarity of structure" may be used, for example, to refer to similarity of complexity such as is found, for example, in one's own pair of feet, each having the same number of bones but the relations among them in one foot being somewhat different from those in the other. In this sense, "similarity of structure" means "having the same number of components". In this sense a hundred trees systematically planted in a grove and a hundred

potatoes scattered haphazardly on a barn floor are also two classes having similarity of structure. This sense of "similarity of structure" corresponds to the first sense of "structure", namely, that of having a plurality of parts or members or terms.

The sense of "similarity of structure" which corresponds to the second sense of "structure" is, however, the most important one and the one most difficult to define carefully. Just as "structure" in its second sense refers to the pattern of relations by which the components of a complex are arranged, whether the relations be nonordering or ordering, so also there is a sense of "similarity of structure" which refers to a relation between two structured classes α and β equally well whether α and β be themselves structured by nonordering relations or by ordering relations and whether α and β be imaginatively similar or not. To the definition of this fundamental and comprehensive sense of "similarity of structure" attention now is turned. Once it has been formally defined and illustrated, third and fourth senses may be defined as species of it, in the third sense where the structuring relations of α and β are nonordering and in the fourth sense where the structuring relations of α and β are ordering relations.

According to Russell, two classes α and β may be said to be similar with respect to structure in the most comprehensive sense if, and only if, all of the following conditions are fulfilled :

- (1) Members of class α are related to one another by a relation P.
- (2) Members of class β are related to one another by a relation Q. (HK, p. 254 : ". . . structure always involves relations : a mere class, as such, has no structure." Cf. AMR, p. 259.)
- (3) Each member of class α corresponds to one and only one member of class β and *vice versa* (IMP, p. 54 ; AMR, p. 250 ; HK, pp. 254, 255, 474). This relation of correspondence holds when, and only when, there is at least one one-one relation S which holds between the members of α and the members of β and which at the same time preserves the structuring relation P in class α and the structuring relation Q in class β . This one-one relation S is the correlator of the two similar classes (IMP, p. 54).

- (4) Whenever two terms a_1 and a_2 in class α have to each other the relation P, then the two corresponding members of β , b_1 and b_2 , have to each other the relation Q, and *vice versa* (AMR, p. 250 ; HK, p. 254).

The first and second requirements insure that each collection or complex compared is not merely a class but rather a class with a structure. The third requirement provides that there shall be some one-one relation S holding between the members of the two structured classes. This requirement that there shall be a

correlator relation S is the crux of the concept of similarity of structure. The fourth guarantees simply that the structured classes are so related that from a knowledge of the relation of two terms in either complex one can infer the relation of their correlates in the other complex (IMP, pp. 55, 61-62; HK, pp. 254, 474-475).

As Russell has emphasized, whenever this complex situation occurs, where there is a correlator relation between two structured classes, the two structuring relations will always have exactly the same logical properties, even though P and Q may be different relations (AMR, p. 251). For example, if either of them is transitive, the other is; if either is connected, the other is; and so it goes, throughout the catalogue of logical properties of relations (IMP, pp. 54-55). Hence, if one of the relations is an ordering relation, then the other is; and if one is nonordering, then so is the other.

This last consideration shows how third and fourth senses of "similarity of structure" may be defined merely by specifying whether the relations P and Q are ordering or nonordering. In requiring that the classes α and β , said to be similar in structure, shall themselves each be structured by a nonordering relation, one has specified a third sense of "similarity of structure". When it is required that α and β be structured by an ordering relation, this gives rise to a fourth and very strict sense of "similarity of structure".

These two senses are mutually exclusive of each other but are species of the second comprehensive sense. Hence, every instance which exemplifies the second sense of "similarity of structure" will also exemplify either the third or fourth but not both. And every instance exemplifying either the third or fourth will also exemplify the second comprehensive sense.

At the same time it can be seen how the third and fourth senses are derivative from the more comprehensive sense defined above, are species of it, and therefore are less fundamental than it.

Also, it is obvious that, in this definition of "similarity of structure" in the second, comprehensive sense, the rigid requirement (often found in HK, e.g. pp. 254, 255) that the two relations P and Q giving structure to the two classes α and β and making them similar in structure must be ordering relations has been avoided.

The reason for this correction and the importance of it have already been suggested. In *Human Knowledge*, when Russell defines "similarity of structure", he demands that each structured

class be ordered (HK, pp. 253, 255, 461; IMP, pp. 58-61). But when he *applies* the concept of similarity of structure in important epistemological problems as well as in some of his expository common-sense illustrations, he regularly uses, and indeed is forced to use, the concept of similarity of structure in the looser, more comprehensive sense defined above rather than in the strict fourth sense of similarity of structure between ordered classes. This stringent definition is thus too narrow to cover the cases that he treats with it.

For example, in this comprehensive sense defined above, which applies alike to pairs of complexes whether their structuring relations are ordering or nonordering, Russell's favourite examples of a map and the terrain mapped (HK, p. 253), a photograph and the object photographed (PHI, p. 131), and a visual percept and its physical cause (PHI, pp. 253-254), may all be said to exhibit similarity of structure. If, however, "similarity of structure" were defined tightly by requiring that the structuring relations be ordering relations, these important cases would fail to satisfy the definition. There would thus be a sharp discrepancy between Russell's formal definition and his application of the concept of similarity of structure.

When the demand that P and Q structuring class α and class β shall be serial relations is removed, this discrepancy between the concept formally defined in *Human Knowledge* and that actually applied disappears in favour of a more liberal and inclusive concept applicable to all the cases to which Russell seeks to apply it. As the definition has been developed here, therefore, P and Q may be ordering relations, but need not be. This is in keeping with Russell's earlier and superior formulations of the concept of similarity of structure (in IMP and AMR), which have been *the basis for my definition of it in the second, comprehensive sense*.

II

Now that the purely logical concept of similarity of structure has been defined, it may be illustrated by examples, first of common-sense cases and then of philosophy, to which the concept is applied by Russell.

Two copies of *Hamlet*, for example, are similar with respect to structure (HK, pp. 466-467). Also, a printed score of a piece of music is similar to its heard melody with respect to structure (HK, pp. 255, 461). A map is similar in structure to the terrain mapped (PHI, p. 131; AMR, p. 249; HK, p. 253). Also, in a

perfectly organized and completely catalogued library the author index catalogue is similar to the collection of books with respect to structure. A man may be said to be similar in structure to a life-size statue of him, to a three-dimensional colour photograph of him, to a colour portrait of him, to a black and white photograph of him, to a pencil sketch of him, to a black silhouette picture of him, and even to a shadow of him (PHI, p. 131; HK, pp. 463-464). Two human skeletons may be said to be similar in structure (HK, p. 461). Anyone of these and of numerous other common-sense examples will satisfy the conditions of Russell's concept and therefore illustrate it.

It will be obvious upon inspection that the cases which I have chosen fall into three very different and mutually exclusive classes. In the cases first mentioned the entities said to be similar in structure are in each case both linguistic entities. In the cases last mentioned the entities compared are in each case nonlinguistic in character. In the others the entities compared are mixed: In each, one of the entities compared is linguistic in character while the other is nonlinguistic.

Cases of importance for philosophy, examples of which may now be given, also belong to one or other of these three exhaustive classes. Notice of these examples will suggest the fundamental importance of the concept of similarity of structure to Russell's philosophical edifice.

Two or more statements, for example, may be said to have the same logical form. This is the same as saying that they are both linguistic occurrences similar in their structure (HK, p. 253).

But if one says, as Russell does (HK, p. 115), that a true basic proposition, for example, is similar in structure to the observed fact it reports, the entities compared in this case are of two different kinds, of which the first is a linguistic expression, while the second is a nonlinguistic occurrence. Moreover, the suggestion that philosophical logic has as part of its function the creation of an inventory of the possible logical forms of fact from which one may with the aid of observation select some which correspond to the facts (PP, pp. 229-231; KEW, pp. 60-69; ML, pp. 112-124; PHI, pp. 296-297) is itself based upon the idea that some properly constructed *statements* may be similar to some *non-linguistic occurrences* at least with respect to structure. Readers of Russell will recognize at once that this use of the concept of similarity of structure as a technical device for connecting logic with fact runs very deep and strong in Russell's philosophy throughout his entire career. His reconstruction of the correspondence theory of truth, for example, is achieved by use of the

concept of similarity of structure for describing the relations between a basic proposition and observed facts.

Russell's use of this concept for describing cases important for philosophy where the entities compared are alike nonlinguistic occurrences is even more conspicuous.

A memory image is said to be similar in structure to its memory object (AMR, p. 266; HK, pp. 114, 422-423, 424). Likewise, an expectation and the event expected are, in veridical cases, roughly similar in structure (HK, pp. 96-97, 427). Moreover, when two observers of the same physical object are visually perceiving it, there are not only differences between their visual contents but also some degree of similarity of structure among them (HK, p. 171), which, in fact, is a basis of the so-called objectivity of perception (PHI, pp. 154-155, 463; HK, pp. 171, 208).

What is more, each perceiver's visual percept of a given physical object is said to be in veridical perception similar in structure to the object perceived (SO, pp. 83-87; AMR, pp. 249-253; HWP, 833-834). Also, in a case of veridical visual perception, there is said to be a causal chain of complex events which proceeds from the physical object through physico-physiological processes to the percept of it, and each complex event in the chain is said to be roughly similar in structure to every other event in the approximately independent causal chain connecting a percept with its causal ancestor (HK, pp. 209, 469, 473-475, 490-491). Moreover, the whole physical universe is said to be traversed throughout by approximately independent causal chains of events (HWP, p. 833; AMR, pp. 322-323; HK, pp. 207-209), such that each event is approximately similar in structure to every other in its chain, whether the chains terminate in a perceiver's nervous system or not (HK, pp. 207-210, 467, 468, 472-475).

What is more, any event, such as a rapid sequence of explosions in a given spatio-temporal region, is said both to originate and to send out radially in all directions relatively independent causal chains of events, in each of which chains the spatio-temporally continuous events are similar in structure to one another and to the central original event which is their causal ancestor (AMR, pp. 258, 322; HK, pp. 193-194, 228, 469-471, 471-472, 487, 492).

It is this complex structural assumption which constitutes Russell's structural postulate (HK, pp. 491-492), and which, at least since he published *The Analysis of Matter* in 1927, has constituted the very core of his doctrine of causality, his theory of perception, and his logic of nondemonstrative inference, in a word, the most central and most original portions of his maturest

epistemology and metaphysics. To see how important this structural postulate has been for Russell, especially since 1927, one needs merely to glance at his major writings since that time. (See, for example, AMR, pp. 226-228, 249-271, 316-317; PHI, pp. 131, 155-158; SO, pp. 83-87; IMP, pp. 24, 206-207, 398-399, 438; RC, pp. 683-684, 701-703, 706, 707, 716, 718-719; HWP, pp. 673-674; HK, pp. xiii, 228, 460-475, 491-492.)

We must not, however, fall into a familiar error of Russell readers by supposing that this one conspicuous and staggeringly ambitious use of the concept of similarity of structure, namely, to formulate his structural postulate, is the only use made of the concept by Russell. Rather, it is but one case of his using the concept to indicate relations among entities which are alike non-linguistic in character. My classification presented above has shown that there are many more important uses made of it by Russell even for comparing nonlinguistic entities, and that, in addition to these, still many more uses are made of it for comparing entities that are alike linguistic, and yet many more fundamental uses are made of it for comparing mixed cases, where one term in the comparison is a linguistic expression and the other is a nonlinguistic occurrence.

In fact, so fundamental and pervasive is Russell's use of this concept in *all his periods* of philosophizing and throughout each of his systems developed in *each of his major periods* that one might well survey most of his philosophy since 1912, when he published *The Problems of Philosophy*, from the standpoint of his uses of the concept of similarity of structure. As a result of such an approach to Russell's philosophy one would in fact omit little of importance.

By this classification of the different kinds of cases to which Russell applies the concept of similarity of structure I have sought to achieve three expository results: (1) to illustrate the concept itself so that its applicability to cases of common sense and philosophy may be imaginatively seized; (2) to call attention to its fundamental and recurrent use by Russell throughout all his philosophy; and (3) to emphasize, by my threefold classification of the diversity of kinds of examples to which it correctly applies, how enormously abstract, general, and comprehensive this purely logical concept of similarity of structure is.

If space allowed, it would be helpful to select typical examples of Russell's and to analyze them meticulously in order to note how they fulfil each of the conditions of Russell's purely logical formulation of his concept of similarity of structure. Instead of doing this, however, I should like it admitted, for the sake of

getting on with the critical task in hand, that in fact Russell's examples will satisfy the conditions of his purely logical definition. Anyone can convince himself by a little attention that it does apply to them.

In using my classification of Russell's examples to emphasize the extremely abstract, general, and comprehensive character of Russell's concept, I not only have been executing my duty as an expositor but also have with methodical maliciousness laid the foundations for my critical evaluation of the alleged usefulness of the concept of similarity of structure for purposes of philosophical analysis.

III

This concept of similarity of structure appears at first to be not only precise in formulation but also extremely useful in its applications to the problems Russell wants to solve by use of it. However, when it is inspected more closely, its alleged and apparent usefulness for philosophy becomes very doubtful. In examining this concept of similarity of structure and Russell's uses of it, I have reached the following critical conclusion : (a) In its precise logical sense formulated by Russell, the concept of similarity of structure, though it is logically impeccable, is wholly useless for each of the many purposes to which Russell tries to adapt it, because the concept is so general that any statement whatsoever asserting of any two or more given wholes that they are similar with respect to structure not only will be true but also will be tautologically true and thus will convey no factual information about them. (b) If Russell's purely logical concept of similarity of structure is to be useful as Russell wants it to be, sharp empirical limitations must be imposed upon it. (c) It is not at all clear, however, what remedial limitations may be successfully imposed upon it. (d) Moreover, even if such empirical limitations were developed, the resulting modified concept or concepts, being empirical, would be altogether different from Russell's purely logical concept of similarity of structure and acceptance of them would probably make it a superfluous piece of technical apparatus.

This criticism of the alleged usefulness of Russell's definition for purposes of describing cases significant for common-sense and philosophical discourse may be substantiated by an argument developed in three short steps.

■ First of all, any two similar classes, that is, any two classes having the same number of members, will satisfy the conditions

laid down by Russell's definition of "similarity of structure". This point may be best made by an example which is trivial and yet satisfies the four conditions of Russell's definition.

One may go into a parking lot, haphazardly label a dozen automobiles, and, leaving them in their positions, regard them as a class of elements with a structure. The structure of this class could be defined in many ways; the simplest would be to indicate the relative spatial relations of the cars selected. Suppose now that one, going home and seeing a child's box of a dozen building blocks, haphazardly tosses them one at a time upon the floor of the playroom. One may then, as in the case of the automobiles chosen at random, regard these scattered blocks as a class of elements whose structure is to be defined. Similarly, one way of defining one set of relations constituting a structure for this class consists in describing the various spatial relations among the various blocks.

But what is interesting, though at first surprising, is that these two classes of elements haphazardly scattered satisfy all four of the conditions contained in Russell's general definition of similarity of structure. First of all, it is obvious that the members of the class of cars, class α , have to one another specifiable spatial relations. It is also obvious that the same is true of the class of automobiles, class β . In the third place, there is any one of many one-one relations enabling one to correlate one and only one member of α with one and only one member of β , the simplest of which would be to assign a natural number from 1 to 12 to each number of each class. In the fourth place, it is obvious that whenever, for example, a_1 has its spatial relation to a_{12} , whatever that relation be, b_1 has its corresponding relation to b_{12} , whatever that relation be; and the same holds for any two members of either class and their correlates in the other class. Consequently, this class of haphazardly arranged automobiles and this similar class of haphazardly scattered blocks are similar with respect to structure, as this concept has been defined by Russell.

Likewise, any two similar classes whatsoever can be shown to be similar in structure in Russell's purely logical sense meticulously expounded above.

The second consideration which enters into proving that Russell's concept of similarity of structure applies equally to all wholes and therefore is too general to be useful for the purpose to which he tries to put it concerns wholes.

Consider first that any single whole whatsoever may be divided into any number of parts chosen at will. In the case of classes,

membership is automatically determined when the property defining the class is designated. But in the case of wholes there is opportunity for analyzing the whole, either physically or merely in attention, into whatever number of parts one may wish to demarcate. This elementary fact about wholes is well known to any mother who has baked a pie for her family but then made it do for dessert for unexpected guests as well. But it is a fact easily overlooked by philosophers.

Moreover, if any whole can be divided into any number of parts, it is equally obvious that any two or more objects can be divided into the same number of parts. The Prime Minister's desk at Number 10 Downing Street and the desk of the Vice-Chancellor of Hong Kong University in Hong Kong, for example, can be exhibited as having the same number of parts. Moreover, we know that this is the case without any observation of either desk and regardless of the characteristics of both desks. We know it because we know that any two wholes whatsoever can be divided into the same number of parts.

But when two wholes are divided into the same number of parts, the two collections of parts, having the same number of members, are two similar classes. Similar classes simply are classes with the same number of members.

What this second set of considerations has shown, therefore, is that any two wholes whatsoever may be so divided into parts that they become two similar classes.

Now, from these two considerations, that any two wholes may be exhibited as two similar classes and that any two similar classes satisfy the conditions of Russell's purely logical definition of "similarity of structure", the third step in the argument follows, namely, that *any two wholes whatsoever can be made to satisfy the conditions of Russell's purely logical concept of similarity of structure*. This is the crux of the critical conclusion that was to be established.

I wish to acknowledge indebtedness to Professor W. V. Quine for calling to my attention that *any two similar classes will satisfy Russell's definition* and to Professor Donald C. Williams for help with the point that *any two wholes can be divided into two equal classes*. Their help came (in January, 1950) when I had already become disillusioned with Russell's uses of the concept of similarity of structure for its excessive generality and gave an additional and essential definiteness to my less precise and less fully generalized criticism of the alleged usefulness of Russell's concept. The concept turned out to be even more general than I had at first realized, and I have devised my argument above

to show how utterly general the concept is. I have also subsequently benefited from discussing my criticism with a former graduate student in my Russell seminar at Berkeley, W. C. Clement, who in the summer of 1950 helpfully read an earlier hundred-page draft of this article.

What follows from my criticism needs to be stated with generality and with care.

It does not follow, of course, that Russell's concept of similarity of structure fails to apply to the important cases of common sense and philosophy to which he applies it. It does apply to them.

What does follow is that the concept, which is intended by Russell to apply descriptively with peculiarly discriminating relevance to cases particularly important for common sense and philosophy, applies equally well to them and to trivial cases. Consequently, it fails to differentiate the important cases from the trivial ones.

The reason why Russell's concept applies to important and trivial cases alike is also obvious from my criticism. To say of any two classes of equally many elements or of any wholes whatsoever that they are similar with respect to structure, far from asserting something important, discriminating, or informative about them, is to utter a tautology applicable to all similar classes and therefore to all wholes. In a word, to say of two similar classes α and β that they are similar with respect to structure turns out to be identical with saying of the two similar classes α and β that they are similar, that is, that two classes α and β having the same number of members do in fact have the same number of members. Statements asserting similarity of structure of any two wholes obviously can also be similarly reduced to tautologies by the method indicated above.

It also follows from the obviously tautological character of statements asserting of similar classes that they are similar in structure that such statements, because they are tautological, convey no factual information whatsoever about the classes or wholes said to be similar in structure, *so long as one adheres in such statements to Russell's purely logical concept of similarity of structure.*

If the conclusion of my argument is correct, then it also follows that from the knowledge that two entities are similar in structure and that one of them has some specifiable empirical characteristic, nothing whatsoever can be inferred concerning the empirical characteristics of the other. For example, no matter how intimately I may know the qualitative and relational features of my

percept of my table, and no matter how assured I may be that my percept of my table and my table are similar in structure in Russell's strict sense of this expression, still there is nothing peculiar in this assertion of similarity of structure which enables me to infer any empirical feature of my table, since it is equally the case that my percept of my table is similar in structure to the Vice Chancellor's table in Hong Kong, to the Prime Minister's table in London, and to a cherry pie baked for the First Lady in Washington.

This implication mentioned last is merely a hasty suggestion of but one of the many consequents important for evaluating Russell's philosophical uses of the concept of similarity of structure which follows from my criticism of its alleged usefulness for solving fundamental problems in philosophy. It is one to which full attention will be directed in a subsequent article.

To be sure, there is supposed by Russell to be a much more intimate resemblance, for example, between a visual percept and its causal ancestor than there is between a visual percept and some physical object not causally related to it; and in supposing this, Russell is doubtless correct. But what precisely are the relevant respects in which the important cases such as my visual percept and its causal ancestor are said to be similar in structure and the trivial case such as my present visual percept and the Prime Minister's desk are not? That is the question.

Russell tries to indicate the relevant resemblance by asserting that there is a similarity of structure between the visual percept and its causal ancestor. But the point to my argument is that, in trying thus to fix not merely this but also other important empirical peculiarities and resemblances characterizing experience, language, and knowledge by use of his *purely logical concept of similarity of structure*, Russell has failed and that he has done so because statements asserting of any two similar classes or of any two wholes that they are similar in structure are tautologically true.

Russell's purely logical concept therefore is criticized not for failing to apply to the important cases that justly concern him but rather for applying to them and to myriads of trivial cases equally well, tautologically, and therefore fruitlessly.

IV

If this criticism of Russell's uses of the concept of similarity of structure is correct, then one may evaluate Russell's system

in either of two ways. One may conclude that Russell's concept of similarity of structure is irremediably useless, that his system, permeated with this concept, is beyond repair, and that therefore the system as a whole is to be rejected. Or, one may at least hope that the concept of similarity of structure may be reconstructed in ways that will perchance make it usefully applicable to the problems Russell wants to solve with it.

Since the latter attitude is more interesting, I prefer it. On a subsequent occasion, therefore, taking the results developed here for granted, I shall explore the question whether Russell's purely logical concept of similarity of structure may possibly be severely modified by imposing sharp empirical limitations upon it which might then make it significantly applicable to problems in epistemology and metaphysics. I shall in the sequel take as a test case Russell's structural postulate, central to his logic of nondemonstrative inference, as an example both for illustrating my criticism developed here and for experimenting with the question whether this purely logical concept can be rendered capable of the functions assigned it by Russell throughout his philosophy of nature and knowledge.

Meantime, if my present criticism of the alleged usefulness of the purely logical concept of similarity of structure for solving philosophical problems be correct, then this result appears to cast fundamental doubts upon at least three varieties of recent outstanding philosophical adventures.

The most obvious direction in which my criticism points is toward the inadequacy of formulation of Russell's philosophy at most if not all those fundamental junctures, surveyed in part in Section II, where he leans heavily upon the concept of similarity of structure.

Moreover, it was observed at the outset that use of the concept of similarity of structure, which is identical with the concept of isomorphism, for approaching problems of epistemology and metaphysics, far from its being restricted to Russell's system building, is also used confidently by many other recent philosophers of eminence, of whom Wittgenstein, Carnap, Schlick, Nagel and Cohen, Weyl, and Eddington are but a few examples. The result, therefore, of my criticism of this concept seems to suggest that this particular piece of technical apparatus trusted by competent contemporary philosophical technicians simply does not have for philosophy the importance attributed to it by them.

Finally, the concept whose alleged usefulness for philosophy has been challenged here is one that has been imported directly

into philosophy from pure logic. It has been found, however, to be of no use for epistemology and metaphysics precisely because it is a purely logical concept, one, in fact, that is so pure that it is sterile! This result, then, at least suggests that the purely technical results of modern logic are not nearly so uniquely and immediately and fruitfully applicable to problems of philosophy as it has been the custom lately to suppose. This last result surprises me. But I see no way to escape it yet.

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VI.—DISCUSSIONS

SOME REFLECTIONS ON JØRGENSEN'S REFLECTIONS IN REFLEXIVITY

I PRESUME that reflecting on someone else's reflections on reflexivity cannot be counted as an instance of reflexivity. But Professor Jørgensen's reflections¹ present an interesting phenomenon of a resurrection of an old controversy. De Witt Parker, back in the early 1930s, also tried to prove that reflexive relations do not exist. Jørgensen goes further; he tries to show not only that there are no reflexive relations but, indeed, no reflexive phenomena at all. Of course, if Jørgensen wishes, in the interest of avoiding certain paradoxes, to surrender reflexive phenomena and especially reflexive relations, he is entirely free to do so. I presume that if he wished to work out a logic along these lines it might be possible to come out with some kind of system that would look very different from the usual logic but might, like non-Euclidean geometry, be perfectly consistent. Whether or not it could properly be entitled "logic" I will not question here. But it can be demanded of Jørgensen that if he is to surrender reflexive relations he should be aware of the consequences and try to do something about them. Whether or not the consequences of the denial of the principle of reflexivity are more drastic than keeping some of the paradoxes, I will leave to Jørgensen to decide. Here I want merely to indicate some of these consequences.

First there would be the following result. In the theory of aggregates, the relation of similarity is defined as a relation such that there exists a (1, 1) correspondence between the elements of two aggregates. If we say with Jørgensen that no reflexive relations exist, then 'similarity between aggregates' must be so defined that no aggregate can be similar to itself. Since the definition of cardinal number depends upon similarity, the number of an aggregate could not be equal to itself. Suppose we try to prove that the cardinal number a_1 is equal to a_2 and attempt to do this by showing that the aggregate A_1 is the same as A_2 . On the basis of the rejection of the reflexive relation, similarity would then not be reflexive, and A_1 could not be similar to A_2 if ' A_1 ' and ' A_2 ' were two different names for the same aggregate.

Then there is the further consequence that we would need to place restrictions on the law of transitivity in whatever area it is used. For example in the theory of aggregates if we show of aggregate A and aggregate B that "A is similar to B" and also that "B is similar to A" we would not be able to infer that "A is similar to

¹ Jørgen Jørgensen, "Some Reflections on Reflexivity," MIND, lxxii, N.S. No. 247 (July 1953), 289-300.

A" since there can be no reflexive relations. This would virtually mean that most transitive relations lose their meaning when certain substitutions are made. This would be true also of "implies" and we would need to say that no proposition can imply itself or be equivalent to itself. Only names of propositions could be equivalent or be said to imply themselves. But if this were granted, then Jørgensen would have to admit that reflexive relations do exist but only in a language (a logic) in which names alone occurred. This, it seems to me, would greatly complicate matters and be hardly worth it in comparison to the advantage incurred by getting rid of the Russell paradox.

Now these are but two examples but, I believe, important ones. However, Jørgensen may be using 'relation' in some sense that I do not understand. Consider that he says "Either an object is the same object at various points of time or space, and if so it cannot have any relation to itself, or the object is not the same . . ." (p. 293). One would ordinarily interpret ". . . is the same as . . ." as a relation in which the same name can be placed. But Jørgensen says that if an object is the same, *i.e.* has the relation "is the same as" to itself, then "it cannot have any relation to itself" and this would presumably mean it also cannot have the relation "is the same as". It cannot be urged that Jørgensen refers to objects because he specifically refers to "object or term" a few lines below. If in order to clarify the matter we are to make a distinction between the expression of a relation and the relation, then possibly Jørgensen is saying that we can write down expressions that intend reflexive relations but there are no such relations. So "X is the same as itself" is but an expression for which there is no corresponding relation. This of course goes counter to much usage even if we do not often have occasion to use the expression. There are such usages, however, as "He remained true to himself through all temptations". This has meaning and does involve a reflexive relation and not merely an expression of one.

Let me consider another expression used by Jørgensen. "To be sure, we can speak of two occurrences or two appearances of one and the same object, as well as we can speak of two objects being identical in certain respects, *e.g.* when they have the *same* property or are having the *same* relation to something. That, however, does not mean that there are two properties or relations that are identical but solely that the same property belongs to both objects . . ." (p. 292). Now one might ask is not being the *same* a reflexive relation? We surely say that A in this occurrence is the same as A in that. Even if the occurrences are different, is not A the same as A? Or are we to be prohibited from saying this at all?

With all these difficulties, it would appear to be the more sensible programme not to say as Jørgensen does that "Any relation presupposes at least two terms . . ." (p. 293) and later that "A class, therefore, must contain at least two members in order to be a class"

(p. 296). Of course, as I said at the start, if Jørgensen wants to talk this way he can do so provided he will take the consequences, *i.e.* that he be willing in the interest of avoiding the paradoxes to enlarge his language in a most cumbersome fashion.

I have not discussed his proposed solutions of the paradoxes because it appears to me that it is better to tolerate them at present than to accept all the results of rejecting reflexive relations. In particular, however, Jørgensen's comments on "this sentence is false" seem to me to be quite acceptable but do not necessitate the drastic rejection he proposes. It is sufficient, I believe, to point out that 'false' is not a predicate for expressions like 'this sentence' but only for complete sentences. In any case, the fact is that while reflexive relations seem to occur quite frequently, the paradoxes are met only in very unusual circumstances.

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ON USHENKO'S VERSION OF THE LIAR-PARADOX

PROFESSOR USHENKO tells us that he has formulated a version of the liar-paradox which is such as "not to be amenable to the kinds of solution which are given by either Russell or Ramsey". The purpose of this note is to show that his demonstration of a paradox rests on a symbolic fallacy. His argument, which appears in his book *The Problems of Logic*, Princeton, 1941, pages 78-80, is as follows :

All propositions written
within the rectangle of
Fig. 1 are false.

FIG. 1.

Let the expression within the rectangle of Fig. 1 be called a and let f denote the phrase "written within the rectangle of Fig. 1". Then :

- (1) fa
- (2) $\sim(\exists p) \cdot [fp \cdot \sim(p = a)]$.
- (3) $a = (p) \cdot (fp \supset \sim p)$.

Suppose a is itself a proposition. Then if a is true :

$$\begin{array}{c} fa \supset \sim a, \text{ by (3)} \\ \hline fa \end{array}$$

(4) $\sim a$

But if $\sim a$, then (3) gives :

$$(\exists p) \cdot (fp \cdot p).$$

This result is compatible with (2) only if :

(5) a .

The vicious circle—from (4) to (5) and back—cannot be avoided if, following Russell, we declare that a , intended to apply to itself, is not a proposition but a meaningless expression. If a is not a proposition, then :

$$\sim(\exists p) \cdot fp, \text{ by (2).}$$

This means that there are no propositions at all within the rectangle of Fig. 1. But then there are no true propositions there either :

(6) $\sim(\exists p) \cdot (fp \cdot p).$

But (6) is another formulation of " $(p) \cdot fp \supset \sim p$ " which by (3) is equal to a . Therefore a is true. But if a is true, it must be a proposition assuming in agreement with ordinary logic that nothing but a proposition can be true. Thus if a is not a proposition, it is a proposition and *vice versa*. This is again a circle in argument which is just as bad as the vacillation between (4) and (5).

Now note first that '*a*' is said to be a name for the expression within the rectangle of Fig. 1. But if this is so, '*a*' cannot stand alone significantly as a step in a proof for there would then be no assertion whatsoever. (5) therefore cannot be used in the proof. If, on the other hand, we construe '*a*' as an abbreviation of the expression rather than as a name for it, then it would not be significant to place it as an argument to '*f*', which is how it is used in (1). (Regarding '*f*', instead of saying "let *f* denote the phrase . . .", it should have been rather "let '*f*' denote the phrase . . .".) If '*a*' were an abbreviation, (1) would be "All propositions written within the rectangle of Fig. 1 are false is written within the rectangle of Fig. 1". This is certainly non-significant and cannot occur as a step in a proof. On either interpretation, therefore, that '*a*' is a name of the expression or an abbreviation of it, we see that the 'proof' falls through.

The use of '*p*' is likewise not univocal. In (6) for instance, '*p*' is taken to be an argument to '*f*' and therefore is used as a variable for names of propositions and also as a member of a conjunction and therefore as a variable for propositions themselves. What makes the paradox possible is thus due to the double use of the letters "*a*" and "*p*" in the proof.

While the demonstration given by Professor Ushenko is intended to be informal, it will be realised that a careful formalization will not give us anything like the paradox he thinks of. We would simply have the standard version of the paradox which falls subject to the treatment of the theory of types or the theory of levels of languages.

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VII.—CRITICAL NOTICE

The Structure of Appearance. By NELSON GOODMAN. Cambridge, Massachusetts : Harvard University Press, 1951. Pp. xv + 315.

DR. GOODMAN'S book is an essay in the Russell-Carnap tradition of philosophy. Dr. Goodman believes that philosophy has just arrived at that stage in its history when rigorous, systematic treatment is to be substituted for the unsystematic kind of investigation which has prevailed in the past. It has, that is, just turned into a science. The object of philosophy henceforth is to be the construction of formal systems, which are to be interpreted in terms of the non-logical concepts of everyday language. In a philosophy so transformed there will be no room for the disputes whose difficulty of resolution has so impeded the progress of the subject in the past ; they will be translated either into disagreements about some one formal system, for whose solution there will be universally accepted and readily applicable methods, or into differences of interest which will lead to the construction of different, but equally legitimate, systems. This attitude is very consistently maintained throughout the book. No philosopher before 1900 is once mentioned : the only writers who are cited more than twice are Carnap, C. I. Lewis and Quine. Remarks like, "In recent investigations of these problems some very discouraging difficulties have arisen" (p. 306), convey the atmosphere of co-operative scientific enquiry which is assumed to be appropriate to a philosophic discussion.

A system may, Dr. Goodman says, be either phenomenalist or physicalistic ; that is, its basic units may be phenomena or phenomenal qualities, or they may be material objects or physical properties. It is, however, the interests of the builder of the system which decide which it is to be ; neither kind can claim any superiority over the other. On the one hand, Dr. Goodman combats the view that material-object statements could not be explained in terms of phenomenalist statements ; for despite the grave difficulties of such a task, it has not been proved impossible, nor even shown to be harder than the admittedly necessary task of explaining, e.g. statements about electrons in terms of statements about material objects. On the other hand, he makes no counter-claims for his own choice of a phenomenalist system. He can make no sense of the view that must lie behind either claim, that phenomena (or material objects) have epistemological priority. Such a claim would involve that when I had a certain visual experience, I saw what I saw as a patch of red rather than as a bird of a certain kind (or *vice versa*) : and Dr. Goodman cannot attach sense to the notion of 'seeing as'.

Dr. Goodman's system resembles that of Carnap's *Aufbau* in

its general plan. We have certain primitive two-place predicates, which apply to things of a certain kind—the 'basic individuals'. The interpretation of each of the primitive predicates is explained informally; we are also given a general account of what kind of thing a basic individual is. Thus in the *Aufbau* the basic individuals were *Elementarerlebnisse*—cross-sections of the total field of experience in the least discernible segment of time. Dr. Goodman's basic individuals are different: while the system of the *Aufbau* was 'particularistic', Dr. Goodman's is 'realistic'. The former type of system takes concrete entities as its basic individuals, the latter abstract entities. Since so many of Dr. Goodman's philosophical arguments consist in rejecting certain notions as meaningless on the ground that various attempted formulations break down (those, e.g. of sameness of meaning and of epistemological priority), it is surprising that he expects the reader to understand the terms "abstract" and "concrete" without further ado. ("Concrete" is later defined within the system: but this does not help us to understand its use in classifying systems in general.) Crudely speaking, while Carnap's *Elementarerlebnisse* cannot occur at more than one moment, Dr. Goodman's basic individuals—'qualia'—are such things as particular shades of colour, smells, kinds of noise, etc., which can occur repeatedly at many times and places. For the visual field, there are three kinds of qualia: colours, places in the visual field, and times. The choice between a realistic and a particularistic system is made on grounds of convenience; there is no prior reason for preferring one to the other.

Dr. Goodman insists that the whole point in building such a system as his lies in achieving economy—in showing how much one can do on how slender a basis. He does not think that it can be left to the reader to assess intuitively how far this goal has been attained: He seeks instead to devise a method of measuring numerically the 'simplicity' of the basis of a system.¹ This method takes into account only the primitive predicates of the system: Dr. Goodman offers a justification for disregarding the special axioms adopted. In assessing simplicity of basis, he also disregards the relative number of basic individuals that there will be, and of basic statements that will have to be given; he offers no justification for this.

What is the status of an axiom of such a system? Dr. Goodman is justly scornful of a Hilbert-like answer, that the axioms are a kind of definition. He argues that if the assertion that an axiom is a disguised form of definition is to have any substance, it must be possible to strip off the disguise: to frame a definition which will do the same work as the axiom. Dr. Goodman claims, as a result of a device invented by him and Quine,² to have a general method of

¹ This has since been revised ("New Notes on Simplicity" by Nelson Goodman in *Journal of Symbolic Logic*, 1952).

² "The Elimination of Extralogical Postulates", by Nelson Goodman and W. V. Quine in *Journal of Symbolic Logic*, 1940.

doing this. Unfortunately, the device achieves nothing; for the reader has to be satisfied of the truth of the axiom thus disposed of in order to admit the correctness of the definition. A better answer to the question about the status of the axioms in a system such as Dr. Goodman's would therefore be to say that they could be derived from collating the totality of basic statements (of which there will be only finitely many). It would still remain a problem what reasons Dr. Goodman has for presuming in advance that the basic statements would in fact yield the axioms as generalisations.

There is a third classification of types of system, into nominalistic and platonistic: that is to say, into those which do not, and those which do, use class-theory as part of the logical framework of the system. (Like Quine, Dr. Goodman regards the use of predicate-variables within quantifiers as involving essentially the same as the introduction of classes.) The choice here is not free: Dr. Goodman considers that there are compelling reasons for adopting a nominalistic system, since he finds the notion of a class incomprehensible. On the other hand, he does not want in his system to have to do by means of predicates all the work that is done in ordinary language by names other than those which stand for those things which he takes as basic individuals; he wants his variables to have other values than just the basic individuals. As this cannot be done by forming classes, he introduces as part of the logical apparatus of the system the calculus of individuals, an account of which was published in the *Journal of Symbolic Logic* for 1940.¹ This is a system based on the part-whole relation, and containing no null element. The relation here taken as primitive is that of overlapping (having a common part): this is designated by "o". The definition of identity is: "'x = y' for '(z)(z o x ≡ z o y)'". By means of this calculus it is possible to refer to the sum of a given number of individuals. This sum, even in a logic which included class-theory and had a theory of types, would be an individual of the same type as the individuals which were its parts. It is sense to assert that the part-whole relation obtains between two entities of whatever kind, provided only that they are admissible values of the individual variables of the system. It might be thought that the sums that can thus be constructed—e.g. in Dr. Goodman's system the sum of two colours, a sound and a time—would be as unintelligible as classes. But the point about classes which Dr. Goodman finds incomprehensible is the same as that which puzzled McTaggart: namely, that the class which contains all and only the counties of England and Wales is not the same as that which contains only England and Wales, i.e. that class-membership, unlike the part-whole relation, is not transitive. In the calculus of individuals, the sum of two sums of individuals contains the basic individuals which the two sums contain.

¹ *The Calculus of Individuals and Its Uses*, by Nelson Goodman and H. S. Leonard.

In fact, Dr. Goodman's objections to classes have in the past appeared to rest on a more generalised suspicion of 'abstract entities' than this; and this comes out in the last chapter of the book, in which he takes up the position that as a nominalist he is not entitled to speak of the word "many" or of the letter "A". This at first seems curious, because nominalism (as he uses the term) had earlier been presented as an objection only to the technical logical notion of classes, and hence without bearing on ordinary forms of speech: Dr. Goodman had been at pains to point out that his nominalism in no way prevented him from adopting a realistic rather than a particularistic system. Dr. Goodman thinks that we ought to stop saying, "The word 'Paris' consists of five letters", and say instead, "Every 'Paris'-inscription consists of five letter-inscriptions"; and instead of "'A' is the first letter of the alphabet", "Every alphabet-inscription starts with an 'A'-inscription". As a strict nominalist, Dr. Goodman does not mean by "inscription" what is ordinarily understood by 'token': inscriptions, being fully concrete, are actual lumps of matter—e.g. bits of ink. Nominalism in this broader sense boils down to nothing but a simple-minded materialism. Dr. Goodman, like many contemporary philosophers, seems not to have grasped Frege's doctrine that only in the context of a sentence does a name stand for anything.

The calculus of individuals performs another important function. The main problem which Carnap had to face in the *Aufbau* was the definition of qualities as classes of the basic units (the *Elementarerlebnisse*), on the basis of only one primitive relation, that of recognised similarity. The difficulty of doing so centred round the fact that more than two objects, each of which is similar to each of the others, need not all be similar to one another in the same respect. This Dr. Goodman calls the "problem of abstraction"; and he shows that the absence of any machinery in Carnap's systems for dealing with multigrade relations of this kind forced him to depend on a number of empirical assumptions in order to justify his definitions. This Dr. Goodman cannot object to on principle; but he demonstrates that there is considerable probability against Carnap's assumptions, and further that they conflict with one another.

Now the basic units of Dr. Goodman's systems are qualia: these are finite in number, and thus do not form compact series; although Dr. Goodman makes no explicit assumptions about finitude or infinity within the system. Times occur in all sense-fields; auditory place, however, is regarded as a physical property. Dr. Goodman discusses the possibility of taking instead particular brightnesses, hues and saturations, and presumably likewise notes, auditory volumes, etc., as the basic units, but for various reasons of convenience rejects this alternative. The first non-logical primitive which he introduces is "W x, y", which means "x occurs

together with y'' ; e.g. if x is a colour and y a place, x occurs at some time in y , or if x is a time and y a colour, y occurs at x in some place. Two qualia from different sense-fields cannot occur together. "W" is symmetrical, irreflexive and non-transitive. As Dr. Goodman's system is realistic, he does not have to face the problem of abstraction; but he has to cope with the analogous 'problem of concretion'. It would be impossible simply to define a concrete object as a sum of qualities each of which had the relation W to each of the others; for there would be no guarantee that if a colour occurred at a given place and at a given time, it occurred at that place *at* that time. But the calculus of individuals provides a solution in that, without introducing a polyadic relation, it is possible to define a concrete object as a sum each of whose parts occurs with every other of its parts. Thus, if x is a colour, y a place and z a time, $x + y + z$ is a concrete object if not only $W x, y \& W x, z \& W y, z$, but also $W x, (y + z)$, etc. To secure the same result using class theory, one would have to have as primitive not a relation between individuals but a property of classes, which would get a bad mark on Dr. Goodman's simplicity rating.

After some definitions along these lines, Dr. Goodman turns to the concepts of shape and size. These are distinguished from such concepts as colour, place and time, whose instances form the basic individuals of the system, by two main features: (1) the parts of a red individual are red, the parts of an individual which occurs at one moment occur at that moment, and so on, but the parts of a square individual are not all square, nor the parts of a large individual all large; and (2) there is a sense in which size and shape are derivative qualities, in that if one knows what places and times an individual occupies, one knows what are its size and shape, but not *vice versa*. For these reasons, there are to be size and shape *predicates*, but no *names* of sizes or shapes. Dr. Goodman therefore introduces a new binary predicate "Z" to mean "is of the same size" (where "size" refers to number of qualia of *all* kinds): this has to be primitive, since it could be defined only in a system which used number-variables. Spatial and temporal size can be defined only after categorial predicates like "is a place" have been formulated; as also shape predicates, which will however not be confined to spatial and temporal shape—there will be colour-shape as well (*i.e.* the configuration of the colours possessed by an individual).

Why does Dr. Goodman regard colours as objects, but shapes not? With a logic of classes, the colour of this flower can be regarded as the class of (material objects) which are-the-same-in-colour-as this flower, the shape of that penny as the class of material objects which are-the-same-in-shape-as the penny. But if we replace "class" by "sum", the latter identification is illegitimate. From a materialist point of view, therefore, according to which the only objects to be countenanced are those which can be construed as sums of temporal

cross-sections of matter, colours are respectable, shapes not. This explains why Dr. Goodman's nominalism leads him to reject not only classes, but shapes, letters and words; although his system is not in fact particularistic, he allows in it colours, places and times as entities, since they could (in another system) be constructed out of irreproachably concrete entities, but does not allow shapes as objects, since there is no such way—without classes—of constructing them. All that this in fact shows is that the logic of classes, for all its difficulties, portrays more faithfully the way in which we do form concepts than a logic based on the part-whole relation.

What is the status of the basic statements out of which Dr. Goodman's system is constructed: in what sense are they given to us? I judge, say, that the (phenomenal) colour which I see in a certain visual-field place at a certain time is the same as the colour which I see in another place at the same time, or saw at another time: what is the status of such a judgment?—is it, for example, open or not open to correction? Dr. Goodman regards these judgments as incorrigible, and explains this by saying that they are *decrees*: I am *laying it down* that "same" or "green" is to apply here. But although no-one can demand that I abandon any given one of these decrees, I may in fact find reason to abandon it. I might find that I could hang on to all the decrees I had made only by giving up what Dr. Goodman calls "the exceedingly useful principle of the transitivity of identity". In such a case, I should drop at least one of my previous decrees; though it would be up to me which one.

Later in the book, however, Dr. Goodman turns to deal with the concept of order. For this purpose he introduces a new primitive binary predicate "*M*" to mean "matches", i.e. "is not noticeably different from". "*M*" is non-transitive and so does not imply identity. It provides a means of defining categories, since two qualia belong to the same category if and only if the ancestral of *M* holds between them. The ancestral is not directly definable in a nominalistic system, but Dr. Goodman circumvents this difficulty: he defines a "clan" as an individual which cannot be divided into two parts such that no quale in one part matches any quale in the other—i.e. an individual all the qualia of which belong to the same category; he then defines a "category" as a most comprehensive clan. He admits that categories as thus defined are not necessarily identical with our ordinary categories: for example, were there a colour which matched no other colour, the sum of all colours would not be a category in the sense defined; but this can be rectified by *ad hoc* measures. Any particular category may be defined by reference to some peculiarity of order which can be discovered and specified. There follows a complicated and interesting series of devices by means of which the concept of order is treated.

In introducing "M", Dr. Goodman adverts to the well-known crux of phenomenism, that though two qualia may match exactly, it may turn out that one does and the other does not exactly match some third quale. If then we were forced to take matching as the same as identity, we should be forced to conclude that identity in this field was not a transitive relation; this contradiction would destroy the very possibility of talking of qualia at all. Dr. Goodman therefore offers the suggestion, and adopts it as a formula of the system, that quale x is the same as quale y if and only if x matches all and only those qualia which match y . The situation is now quite altered. If I say that the colour I see matches the one I saw five minutes ago, I am not open to correction: nor—as matching is not transitive—is there any reason for my withdrawing the statement later. But if I say, on the basis of judging that two colours match, that they are the same, then I am open to correction; for my statement is to the effect that each colour matches all and only those colours which the other matches, and this might well turn out to be false. Thus in place of Dr. Goodman's decrees, which had a modified incorrigibility, in that they could conflict with one another, we now have judgments of identity, which are quite simply corrigible, and judgments of matching, which will be among the basic statements, and which it at first sight appears that we have no reason to suppose to be open to correction.

Dr. Goodman uses the relation of matching as the basis for the ordering of qualia. He uses it with some confidence that it will produce either the same results as have been obtained by other methods—such as the colour sphere, or the distinction between hue, brightness and saturation—or recognisable improvements on these results. If judgments of matching are incorrigible, there would seem to be no reason for this confidence. There seems to be no place for a mistake of memory: a mistake, that is, not about what colour a thing *was*, but about what colour it *looked*. We have a place for speaking of such a mistake in ordinary language in that there are circumstances in which one would say, "It certainly must look the same as it did before, and *therefore* you must be remembering it wrong". In a discussion of his work held at Oxford, Dr. Goodman explained that he did allow for what we should normally call a memory-mistake, and that this was the ground for his confidence in the ordering of qualia in the basis of the matching relation. He wanted, that is, to explain "matches" for colours in exactly the same way as we explain in ordinary life "looks just the same colour as". I can find no hint of this in the book: his discussion of decrees—which in any case is seen to have been misleading when the notion of matching is later introduced—suggests quite a different approach. However this may be, Dr. Goodman's system now no longer appears to be phenomenistic in the ordinary sense of the term; since, normally speaking, what appears in a phenomenalist language is

that which (it is supposed) could be taught, or grasped, without reference to anything physical.

The whole project of 'constructionalism' is, to my mind, misconceived. The constructionalist's goal is to frame, or at least indicate, definitions. Now definitions, of the sort that occur in formal systems, serve many purposes : but Dr. Goodman takes definition as an end in itself. Definitions are often drawn up with an eye to proofs : if we define the concepts of one branch of mathematics in terms of those of another, we can see whether new principles need to be invoked, or whether the axioms of the latter branch are adequate to the former also. Again, one of the main motives for the reduction of primitives is to simplify the syntax of a system, and in particular to facilitate the proof of metatheorems. A quite different motive is the elucidation of concepts which puzzle us in terms of others which we think we understand, or at least the elucidation of which in turn appears not to involve the defined concepts. None of these motives seems to be operating here.

Dr. Goodman does not suppose that anyone could build up concepts in the way he builds up his system (could, e.g. start with the concept of a quale, and have explained to him the concepts of a colour and a time in terms of the ancestral of the matching relation). Nor does it worry him if in the informal explanation of the primitives (e.g. "matches") the most complicated physical circumstances are alluded to. Dr. Goodman's interer does not lie, either, in merely showing that all the necessary concepts are definable in terms of his primitives : he would not consider it sufficient to say that "temporally precedes" could be defined in terms of some two temporally distinct occurrences. On the other hand, it is not demanded that every concept be actually defined : the definition of "temporally precedes", for example, would have to include names (e.g. of times) whose interpretation would involve knowledge of historical circumstances. The stipulation is therefore that *we*, who do not use the language of Dr. Goodman's system, should be able to work out from the definitions the interpretations of any of its predicates and names, given the interpretation of the primitive predicates and the nature of the basic individuals, *and also* having such knowledge of historical circumstances (what colours have appeared when and where) as we do have. The only point of this stipulation appears to be to preserve at the same time the feasibility and the difficulty of the enterprise.

Despite its mistaken approach, the book will be enjoyed by, and will reward, anyone interested in philosophy and not frightened by logical symbols. Dr. Goodman does succeed in exhibiting by means of his definitions *some* of the logical features of the concepts in question ; and there is also a great deal of philosophical discussion which is not carried on by means of the formal system, but has to be given as a preliminary, either to explain the interpretation of the system, to state its purpose, or to vindicate the principles on which

it is constructed. As MIND is primarily a philosophical periodical, I have paid scant attention to those parts of the book which are rather of logical or of mathematical than of philosophical interest; these sections are the most successful in the book, and will repay close study.

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VIII.—NEW BOOKS

Religion and the Modern Mind. By W. T. STACE. J. P. Lippincott & Co. Philadelphia. Pp. 281. \$3.75.
Time and Eternity. By W. T. STACE. Princeton University Press. Pp. vii + 266. \$3.00.

BOTH these books are written in a clear, straightforward, and lively fashion, and though the first is avowedly more popular than the second they have a common concern with the problem of religious truth. In the first book this problem is approached historically and discussed in the setting of contemporary thought ; in the second book the problem receives a much more systematic and thorough epistemological examination.

In Part II of the first book Stace describes the rise of modern science and examines its challenge to what he has called in Part I "the medieval world picture", something which is more accurately described on page 179 as "the religious view of the world": belief in God and in a universe exhibiting purpose and moral order. The challenge he considers to be three-fold : (a) to religion, (b) to morals, (c) to philosophy. With regard to (a) and (b) he claims that while in each case the new scientific viewpoint had important (and he would say disastrous) *psychological* results, it was always *logically* irrelevant to theological and ethical beliefs. His discussion of (c) is much more detailed and complex and somewhat rambling. After nearly a chapter (chapter 7) on the character of philosophy, he claims that "the history of the modern period" exhibits the challenge of science as a conflict between two kinds of philosophy (p. 147) :

- (i) "Philosophies which express the scientific view of the world" and the examples he gives include Hobbes, Hume, Comte, Schlick, and Ayer.
- (ii) "Philosophies of reaction and protest in favour of the religious view of the world", and the examples he gives here include Berkeley, Kant, Hegel, Bradley, Royce.

Whatever we think of Stace's classification and his more detailed comments, if once we allow the broad distinction he makes, it is clear that to resolve the conflict it implies, one important question to be discussed will be the status of religious truth. For the one side in various ways would attack or ignore it, whereas the other side in various ways would assert its immense significance.

In Stace's defence of religious truth, which is developed much more fully in the second book, there seems to be three main points.

1. *An emphasis on "mystical illumination" or "intuition".*

This is an experience in which, he says in the first book, "all discrimination, difference, multiplicity, are transcended". It is "beyond the understanding". In it "there is no duality" (p. 237). "Negative theology" he says in *Time and Eternity* has the positive function of evoking this experience and the negative function of emphasising that "God is inaccessible to the logical intellect" (p. 139). Incidentally, as he reminds us in a note on page 86, the experience "is misdescribed if it is called an emotion", for there is nothing "subjective" about it—it "transcends the subject-object dualism".

2. A claim that theological doctrines are (in his sense) "symbolic".

He approaches this claim by noting that even if God is "hidden from intellect" (p. 49) theologians, and here Stace is with them, in some way or other do attribute positive predicates to him. How, he asks, do they do this? His claim is "that all religious language must be taken as symbolic and not as literal" (p. 60). The reference of religious language is to the experience it symbolises and evokes. But it is still not clear why anything more than negative theology is needed to do this. If negative theology can evoke the experience, why go further? Not that the working of negative theology is all that straightforward; but do we need to burden ourselves with positive theology as well? How, more generally, does either theology "symbolise"? It is only fair to say that Stace is well aware that his concept of symbolism needs very careful elucidation, but all the same my own judgment is that his discussion does not go nearly far enough.

3. A claim for the "intersection" of theological and other language.

Stace holds, as part of his view about religious symbolism, that the temporal and the eternal, using these words vaguely and perhaps confusedly to describe varieties of language as much as kinds of fact, "intersect" in the saint and to some degree in everybody. "The moment of mystic illumination" when "the two orders intersect" (p. 75) is clearly an epistemological junction of the highest importance. But it is here, where we might look for some positive and constructive suggestions, that Stace is very unsatisfying. If we ask ourselves what light this "intersection" throws on the relation between the "logical intellect" and the ineffable experience, it is difficult to get a clear answer.

At times (though it surely compromises his notion of symbolising as much as his notion of intersecting) he speaks as if the two were logically and factually quite separate. Then he can say that even though "the two orders intersect . . . each is wholly self-contained" (p. 136). Similarly he can claim that "there is no such thing as natural theology" (p. 151). Further, some such view as this is no doubt behind the distinction he draws (and as far as it goes, usefully) between "explanations" in science and theology in chapter 5 of the first book. But if there is this logical separation, why and how justifiably do we use the traditional adjectives about God—the problem which is still unsolved. If theology has no literal meaning whatever, must it not, on Stace's extreme view, become an arbitrary choice between one group of nonsense-jingles and another? It may seem to many philosophers that such a choice is just what their theological colleagues make, but let us hesitate before we say that necessarily they can do no other.

Stace might retort, however, that in many places he shows himself anxious to elucidate some connexion between religious and other languages, and so to throw some positive light on his relation of symbolising. For example, on page 51 he speaks as if symbols give (literally? "directly"? "some glimpse: some hint" of that to which they refer, Again in the chapter on religious symbolism, when discussing the differing adequacy of religious words he approaches quite closely to a hierarchical idea of language. He now argues that positive predicates might be distinguished in adequacy by reference respectively to "an order of being" or to "an order of values" (p. 115). But are these "orders" empirically given, or are they tied up with some absolute idealism, or to be accepted as presuppositions? It is precisely when the

problem is warming up like this, that Stace leaves us to pursue the game ourselves.

Such constructive suggestions as he whispers to us on leaving, are very reminiscent of Hegel, and not least the central claim that the mystical experience is a unity transcending "all discrimination". But may not this be the source of all Stace's difficulties? If once we deny the subject-object structure of the intuition, how can ordinary language ever be used significantly about it, since such language is constructed on a presupposition about experience which the alleged fact denies. We may be the more surprised that Stace, after his vigorous campaign against literalism in the higher reaches, takes this word "unity" so "literally" as to suppose that the experience which it characterises must then be necessarily a uniform blank. On such a view the best theology will be negative indeed; so negative as to say nothing whatever. Is it that Stace's Hegelian and Eastern sympathies are so strong that he is persuaded to deny the subject-object structure of mystical experience even when to do this he must needs surrender the possibility of an intelligible positive theology?

All the same, if anyone seeks a straightforward approach to the puzzles of theological language, with many hints that are worth pondering further, he can do no better than read the second book. We may wish Stace had said more; but if that is criticism, it is also praise. Some books are better not even begun.

I. T. RAMSEY.

Logic and Language (Second Series). Edited with an Introduction by ANTONY FLEW. Basil Blackwell, 1953. Pp. 242. 2ls.

JUST as this volume of essays is complementary to the First Series under the same title,¹ so I would like the present notice to be a kind of continuation of Mr. Holloway's excellent review of the earlier volume, which appeared in MIND for January, 1952. Mr. Holloway pointed out and pointed out quite correctly and forcibly that the orientation of the essayists in that volume (he speaks of it as "the linguistic movement in

¹ I trust the reader will pardon a breach of taste if I append a footnote for the benefit of those who might wish to have at hand a table of contents of the two volumes of this work:

LOGIC AND LANGUAGE (First Series): I. Introduction, A. G. N. Flew; II. "Systematically Misleading Expressions", Professor Gilbert Ryle; III. "Time: A Treatment of Some Puzzles", Professor J. N. Findlay; IV. "Bertrand Russell's Doubts about Induction", Paul Edwards; V. "The Philosopher's Use of Analogy", Margaret Macdonald; VI. "Is There a Problem About Sense-Data?", B. A. Paul; VII. "Verifiability", Dr. F. Waismann; VIII. "The Ascription of Responsibilities and Rights", H. L. A. Hart; IX. "The Language of Political Theory", Margaret Macdonald; X. "Gods", John Wisdom.

LOGIC AND LANGUAGE (Second Series): Introduction, A. G. N. Flew; I. "Language Strata", Dr. F. Waismann; II. "Will the Future be Like the Past?", Dr. F. L. Will; III. "Universals", Dr. D. F. Pears; IV. "Categories", Professor Gilbert Ryle; V. "Is Existence a Predicate?", Professor G. E. Moore; VI. "Every Event Has a Cause", G. J. Warnock; VII. "Incompatibilities of Colours", D. F. Pears; VIII. "Other Minds", Professor J. L. Austin; IX. "On Grading", J. O. Urmson; X. "Historical Explanation", A. M. MacIver; XI. "Mathematics and the World", Professor Douglas Gasking; XII. "Theory Construction", Professor J. J. C. Smart.

philosophy", but this is somewhat misleading as it does not distinguish this English species from the Viennese variety with which, as Flew emphasizes, it should not be confused) is an amalgam of two tendencies: an empirical study of good English usage, and an attempted nullification of traditional metaphysical problemizing based on the results of such a study. To this analysis, which I think essentially sound, and to Holloway's critical stand, to the effect that the nullification of metaphysics is not cogently consequent upon any results of the empirical study of language, I would only add that the matter is not quite as simple as this formulation suggests. In saying this I am not primarily thinking of the obvious truth that the English linguistic movement is hardly a "school" in any strict sense, that, despite its "snugness" (to use Holloway's phonetically suggestive term), it does embrace individuals of rather strikingly different outlook. It is nonetheless properly designated a "movement" (and so far as its members would deny this they are "guilty of a peculiarly vicious sort of parochialism"), and my intention is simply to add to Holloway's characterization of it. It exemplifies, it seems to me, certain further tendencies than those he mentions, tendencies which, perhaps, serve to blur the sharp distinction between the two he discusses.

One of these is a sort of literary behaviourism, by which I mean not merely a behaviourism expressed in decent English (as contrasted with its American forms), but also one which is primarily concerned with linguistic behaviour. Thus, to use a single example, Mr. Urmson, in his clear-cut paper on grading [i.e. evaluating], is able to sort the wheat from the chaff in all three current axiological views, naturalism, intuitionism and emotivism, without apparently any philosophical commitments of his own by insisting that grading is a unique activity. This procedure is rather characteristic: the philosophic issues are tacitly replaced by descriptions of (linguistic) behaviour, and the substitution is not easily noted because the behaviour investigated is either that of the philosophers themselves or of laymen when talking about matters pertinent to the subject of philosophic dispute. If this device were to be justified it would seem to commit one to a sort of behaviouristic materialism (one thinks of George Herbert Mead), to the effect that the world is humans [specifically, Englishmen] talking (gesturing, etc.), including of course as a major subdivision talking in response to the talking of others.

Another characteristic tendency may be labelled "contextualism". This operates to nullify metaphysics as follows: There are no problems, thus *a fortiori* no solutions, in general; all are concrete questions answerable only through a knowledge of particular individuals in their actual situations. So for Dr. Will there is no general issue as to whether the future will be like the past (i.e. whether we may assume the unobserved to resemble the observed); we need only concern ourselves with specific predictions and these are often verified. Similarly for Mr. Pears there is no over-all problem of naming (i.e. of gaining "exit from the maze of words"), but only the special questions of whether this or that name is well-applied, picking out unambiguously a group of designated entities. Likewise for Professor Austin there is no philosophic difficulty about knowing other minds and their experiences but only the special problems involved in knowing the particular emotions of particular individuals on particular occasions. This dodge would also if justified commit one to a philosophic position—a sort of (methodological) nominalism (reminding one of John Dewey).

This supplementation of Holloway's analysis is not intended to supersede nor to overthrow his illuminating distinction between the empirical and the anti-metaphysical tendencies in the English linguistic movement, nor does it claim exhaustiveness in delineating other features of that approach, but it is meant to illustrate its complexity and to indicate why the *non sequitur* so clearly pointed out by Holloway has not always been simply obvious. And though I have drawn my examples from the Second Series they might just as well have been taken from the first.

Now it is my personal conviction that the attempt to nullify philosophical probing and puzzling by an empirical study of good English usage in everyday situations just hasn't come off. Despite the brilliance of some of its practitioners, this cult is losing its appeal and will very soon reach a dead end. Thus it is interesting to note that Holloway found evidence even in the earlier volume of some resurgence of the metaphysical attitude. I think more of this can be discovered in the present one. This may be due in part to the level of the essays in this Second Series, which is more that of professional writing as contrasted with popularization, and in part to the inclusion of several original statements, not previously published.

What I have in mind comes out, perhaps, most strikingly in Professor Waismann's essay on "Language Strata". He contends that 'true', 'verified', 'meaningful', 'logical', etc., are radically ambiguous, changing with changes in their linguistic contexts. This, as just formulated, might seem to be simply another statement of linguistic contextualism, but there is a difference. Professor Waismann restricts his contextualism so that it no longer takes the particular situation (and certainly not the behavioural one) as its frame of reference, but a much larger scale language layer or stratum. Amongst these strata with their different logics and semantics he puts different philosophical modes of speech, e.g. the materialist and the phenomenalist.

This, it seems to me, is exciting and suggestive. Unfortunately, it is also confusing. For he doesn't distinguish such categorial differences of language strata from differences in everyday modes of speech and thought, such as the logic of dreams, of fiction and of waking experience. Indeed, I take it he is explicitly attempting in some fashion to assimilate the former to the latter, thereby producing a complete tolerance between different philosophical lingoes.

Yet there is a difference from the ordinary contextualism of the English analytic school. There are, apparently, for Professor Waismann, only a comparatively few language strata, each with its own characteristic logic. If he were to take one further step, viz. to retain the somewhat traditional view that, to put it linguistically, each philosophic position attempts to furnish a language in which everything empirical in sense can be said, and that the strife of systems is between such supposedly complete languages and is as to which is most adequate and clarifying in its categorial (or if one pleases, its "logical") aspects, then he would have moved beyond the philosophic sterility of much current English analysis. And indeed something like this seems definitely suggested by some of his remarks in his essay in the First Series to the effect that what we see as a fact depends upon the categorial structure of the language through which we see it, and that simple-minded empiricism is too naive a faith. Such remarks at least suggest that different philosophic languages are rivals for the total job of furnishing a medium for empirical description and that some sort of criteria for choosing between them or

of discarding them all but retaining some kind of insight from each should be forthcoming.

There are other essays that leave a similar impression, namely, that their authors are thinking of linguistic analysis not merely as a weapon with which to fight the metaphysical urge but as a tool to help it achieve some degree of satisfaction and clarity. Space limits, however, prohibit further specification.

Though differing in my personal desire, I end with the same question that Holloway did. "Does this raise a problem for the future?" The experts in analysis have used their expertise to reach philosophically sceptical results. Will this change? Will their analyses be "deployed not to oppose, but to elucidate, sharpen, and above all direct, that 'insight' at the findings of which metaphysical assertions seem to be trying to hint[?]" Personally, I have no desire to see this come about; but it is not easy to see anything which would prevent it in principle."

EVERETT W. HALL.

David Hume. By ANDRÉ-LOUIS LEROY. Paris: Presses Universitaires de France, 1953. Pp. 342. [No indication of price.]

This is a very learned and scholarly work. The author modestly describes it as 'un simple sommaire' of Hume's philosophy of human nature. But it is much more than that. It is an interpretation of Hume's philosophy, supported by a very careful and detailed study of the text; and this interpretation differs in an interesting way from those with which most English-speaking readers of Hume are familiar. Rather more than half the book is devoted to Hume's theory of knowledge. But the other parts of Hume's philosophy of human nature also merit their due: his theory of the passions and of sympathy, his moral philosophy, his social and political theories (illustrated by occasional quotations from the *History of England*), his philosophy of religion, his aesthetics, and even his economics. The *Essays* are not neglected. There is an interesting chapter in Part IV about the *Dialogues on Natural Religion*, and the *Dialogues* are also used, earlier in the book, to throw light on Hume's theory of knowledge. The book ends with a brief discussion of the present significance of the philosophy of David Hume.

The exposition follows Hume's text pretty closely. All the same, as M. Leroy says in the Preface: 'the Hume I have presented is *my* Hume; it could not be otherwise.' What kind of a philosopher is M. Leroy's Hume? He is not the 'atomistic' philosopher of the textbooks. He is not a sceptic in the traditional sense. When he said that ideas are copies of impressions, the word 'copy' is not to be taken literally, and we must always bear in mind that impressions of reflection are at least as important as impressions of sensation. Perhaps we might best describe M. Leroy's Hume as the philosopher of the imagination; and the procedures of the imagination are thought of in biological rather than 'mechanistic' terms. The biology, however, is Bergsonian. The key-word is 'spontaneity'. M. Leroy is constantly emphasising the spontaneity of the Humean imagination. Belief is the work of imaginative spontaneity. The imagination is spontaneously orientated towards the future. It spontaneously corrects its errors by means of general rules. (There is an excellent discussion of Hume's doctrine of General Rules in ch. 4.)

Again, we are told that Hume's comparison of the human mind to 'a bundle' must not mislead us. This phrase, considered in its context, has a merely polemical purpose; it is directed against the conception of a static soul-substance (p. 168). Hume's other analogies, of a theatre and a 'republic or commonwealth', are the important ones. He thinks of the human mind as 'an active spontaneity' (p. 169); its nature is 'action, an action without an agent, or rather an action which is the agent' (p. 170). Does M. Leroy distinguish sufficiently between an *activity* without an agent, and a series of *happenings* without an identical substratum?

There is the same stress on activity, and even of effort, in the interesting chapter on Hume's scepticism (ch. 14 'Le Scepticisme Académique'). Hume's scepticism, M. Leroy thinks, did not amount to much more than anti-dogmatism; it was, moreover, a way of life as well as a merely theoretical attitude. It was a canny and cautious 'probabilism', recognising that there are degrees of evidence, and insisting that in our efforts to get knowledge of the world we can never claim finality, but can only proceed by a method of successive approximations. M. Leroy also describes it—using the English words—as 'a tentative empiricism'. (Perhaps 'an empiricism of tentativeness' would be a better phrase for his purpose.)

This probabilism, he says, is 'very close to the spirit of science' (p. 199). We are inclined to reply that it would be but for Hume's doubts about induction. Most English-speaking philosophers think of Hume as the man who discovered the Problem of Induction. Oddly enough, M. Leroy says hardly anything about this. The very word 'induction', so far as I can recall, does not occur anywhere in the book. The only relevant passage is a very brief one on the assumption that the future will resemble the past (p. 189); and all he says about this assumption is that it expresses the imagination's spontaneous orientation towards the future, an attitude which is at first completely unjustified, but is gradually confirmed by its success, though even then it is never more than probable. (It is rather odd to say that an orientation is 'probable'.) But surely Hume himself did not think that the problem of induction was only concerned with the future. He raises it in a completely general form. 'There can be no demonstrative arguments to prove that those instances of which we have had no experience resemble those of which we have had experience' (*Treatise*, Book I, part iii, section 6). Hume was well aware that our beliefs about *past* events beyond the range of our own memories rest on inductive grounds. The *Essay on Miracles* shows this clearly, if further evidence is needed.

It is, of course, just arguable that Hume was not after all a sceptic about induction. The *Essay on Miracles* itself suggests that he was not. Perhaps he would have agreed with his modern disciples that the so-called Problem of Induction is in the end a pseudo-problem, arising from the mistaken assumption that the deductive standard of validity is the only admissible one. The case for this interpretation of Hume has been persuasively argued by Mr. R. E. Hobart in his two articles 'Hume without scepticism' (MIND, 1930). Perhaps this is the interpretation which Mr. Leroy would accept. But he does not say so explicitly. Nor does he mention Mr. Hobart's articles in his bibliography, though he does mention almost everything else which has been written on Hume in English.

H. H. PRICE.

The Theory of Universals. By R. I. AARON. Clarendon Press, Oxford, 1952. Pp. viii + 243. 21s.

PROFESSOR AARON intends his book as a contribution, or perhaps a preliminary, to the theory of thinking. After five historical chapters, he begins his discussion by formulating the question "How do we use general words?" (p. 124). Rejecting the attempt to explain the use of general words by means of the distinction between denotation and connotation, he goes on (in chapter viii) to dismiss the *Ante Rem* or Ideal theory. (He retains, however, a belief that this theory may contain the answer to the question "What is the final explanation of the recurrences in nature?" (p. 242)). Common qualities are next (chapter ix) discussed. Professor Aaron holds that though they do not account for all or perhaps for any of the occasions on which we successfully use general words, nevertheless genuine identities of quality may exist. But for the most part *resemblances* between things are what justify use of general words. Dispositions (chapter x) and concepts (chapter xi) are next considered—Professor Aaron accepts from Hume the view that thinking depends on the more or less unconscious formation, beginning with the earliest years, of habits and skills, including, of course, dispositions to use general words. In the chapter on *concepts*, he considers the place of imagery in conceptual thinking and the relation between mere habit and conceiving in a more developed sense. The phrase 'to have concepts' "covers the explaining to oneself in words what this word or these words mean, so that whenever we describe, whenever we define, and whenever we set out precisely the principle involved in any circumstance, be it a physical law or a psychological or logical rule, we are conceiving" (p. 211). The conclusion of the book is "that though philosophers are free to define the word 'universal' as they choose, the theory most likely to be helpful when we seek to explain human thinking is one for which universals are, first, recurrences and, secondly, principles of grouping" (p. 241).

In discussing Locke, Berkeley and Hume on general ideas, Professor Aaron writes with authority. The other historical chapters are less satisfactory. That on the medieval metaphysics of universals, beginning as it does with a (minor) mistranslation of Porphyry, is too slight to help us much in the solution, or even the formulation, of the main problem, and one has the feeling that Professor Aaron is unwilling to accept from Kant what he readily accepts from Hume.

The history of philosophy having taken the course it has, it was no doubt legitimate to use the noun "universal" as a clue in these inquiries. But the result is misleading and tiresome. Nor are the discussions pushed far enough: What is the difference between identity of quality in two instances of ultramarine and exact resemblance in a certain respect of two ink blots? Why does Professor Aaron choose the word "recurrence" in preference to "similarity" as the key word on the objective side of his analysis? Does it not introduce an unwanted reference to temporal succession? Do we not require a much fuller discussion of the relation of *habit* and *principle*?

Though he strives manfully to bridge the gulf between the conventional philosophy curriculum, with its strong historical bias, and the present-day practice of philosophy in the Universities of Oxford and (shall we say?) Birmingham, one often doubts how far Professor Aaron has succeeded in thinking himself into the position of the authors whose views he considers

and in some cases espouses. There are examples on pages 182 (dispositions and brain traces), 199 (internal accusatives), 227 ("All men are mortal") and 234 (observations as principles). Readers who have acquired any high degree of sensitivity to what is sometimes called logical syntax will suffer agonies at almost every page.

Professor Price's *Thinking and Experience*, of course, did not appear till later. But the close coincidence of their subject-matter makes comparison quite inevitable. For this book the comparison is unfavourable.

D. R. COJSIN.

The Return to Reason. Essays in Realistic Philosophy. Edited by JOHN WILD. Henry Regnery Company, Chicago, 1953. Pp. viii + 363. \$7.50.

THIS large and expensive volume contains original papers by fourteen American philosophers. Within the limits of this review it is impossible to discuss them individually, so I shall confine myself to some general remarks about the book as a whole. The contributors form an organisation known as The Association for Realistic Philosophy, which has existed for five years, so the Editor tells us. Rather like a political party, the Association has what it calls a 'platform' which is printed at the end of the book. This consists of a number of principles and definitions concerning the subject matter and method of philosophy. It is the purpose of the Association to provide a "critical clarification and defence" of these. It is nowadays fashionable to deplore, and even to deny the existence of, sects and schools of philosophy. I have never seen much point in this pretence, and it is refreshing to encounter a group who not only confess that they form a school but who also have the candour to admit and describe their general standpoint. There is, of course, an important paradox involved in this. For it would be argued by many that it is the very task of philosophy to prove the truth (or even the mere significance of) just such general principles as are laid down by these philosophers as their 'platform'. Nevertheless, it is plain that philosophising takes place on the basis of a good many hunches, predispositions and downright prejudices, which seem to be more frequently substantiated than refuted by argument. The activity of philosophy is not only an attempt rationally to reach a standpoint, for it always takes place from a standpoint.

In his introduction the Editor deprecates "the recent flight from philosophy which has left a vacuum which must somehow be filled". This is one more example of the widespread tendency to define philosophy as whatever it is one does oneself, whether it be linguistic analysis or the kind of synoptic metaphysics sought by these writers. Nothing is easier than to patent the title of philosopher and deny it to others by means of a definition. I do not know the reason for this particular form of childishness, although there are some suggestive analogies in recent political history.

The platform of Realistic Philosophy is a curious pot-pourri of Aristotle, Aquinas, Husserl and Brentano. It originates in a dissatisfaction with the two types of philosophy which have dominated America for some years—Positivism (Carnap and Reichenbach) and Pragmatism (Dewey and Lewis). Unfortunately the fundamental objection which these realistic philosophers make against the two rival theories is misconceived,

with the result that there is a good deal wrong with their positive proposals. What these philosophers really object to is, in the words of the Editor, "scepticism about the capacity of disciplined reason to shed any real light on the things that matter most". And again he refers to "subjectivistic, irrational, and relativistic modes of thought which are empirically unsound and culturally demoralising". This is a not unfamiliar charge against empiricism. Undoubtedly philosophers who are dubious about the possibility of metaphysics are reluctant to adopt the traditional rôle of wiseacre. And it could even be argued that this is a 'culturally demoralising' state of affairs, in that those in doubt and despair, receiving no sustenance from the philosopher, are led to consult political, religious and psychological charlatans who do more harm and talk more nonsense than even the silliest metaphysician. Granted all this, I entirely fail to see that any rational case has been made out against scepticism on the ultimate issues which occupy these philosophers. It may be very perturbing, it could even be socially dangerous, that the problems of realistic philosophy are insoluble—but it could still be true. According to the platform, philosophers should be investigating "the basic concepts of first philosophy: being, one, true, beautiful, good (general metaphysics); potency-act, essence—existence, substance-accident and causation (finite metaphysics)". In assuming that these problems are soluble (or even intelligible) these philosophers are begging the most important question with which they are faced. It seems to me that these writers, and also many who would deny that they are building systems, could profitably take to heart the following observation by Robert Musil: "Philosophers are violent and aggressive persons who, having no army at their disposal, bring the world into subjection to themselves by locking it up in a system."

C. K. GRANT.

Received also:—

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IX.—NOTES

At a meeting in Birmingham on the 30th September, a philosophical society was founded which is to be known as the Muirhead Society. Meetings will be held in Birmingham about once a month during the academic year, and will take the form of an address followed by discussion. Enquiries should be addressed to the Secretary, Mr. R. L. Sawers, Lye Green Cottage, Claverdon, Warwickshire.

MIND ASSOCIATION

Those who wish to join the Association should communicate with the Hon. Treasurer, Mr. J. D. MABBOTT, St. John's College, Oxford, to whom the yearly subscription of sixteen shillings (payable in advance) should be sent. Cheques should be made payable to the Mind Association. Members may pay a Life Composition of £21 instead of the annual subscription. The annual subscription may be paid by Banker's Order; forms for this purpose can be obtained from the Hon. Treasurer.

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